# THE PHYSICS

OF

# EARTHQUAKE PHENOMENA

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# PREFACE

HAVING been appointed Thomson Lecturer in the United Free Church College in Aberdeen during the Session 1905–6, I was invited by Principal Iverach to deliver a course of lectures on earthquakes. This book is the outcome of these lectures, which probably are unique in having been the only systematic course ever delivered on the subject in this country.

Since the days when I studied Geology under Sir Archibald Geikie, then Professor in Edinburgh University, I had always retained a strong interest in the many physical problems suggested by geological and geographical facts. Accordingly when, in 1883, I entered on my duties as Professor of Physics in the University of Tokyo, Japan, my interest in seismological questions naturally received a great impetus. With Professor John Milne as a colleague it was impossible for me to escape being to some extent fired by his enthusiasm. A glance through the succeeding pages will show how largely this eminent seismologist has influenced the thoughts which find expression. It was my good fortune to witness the conception and growth of many of his most fruitful ideas, to see how at every turn he appealed to experiment to elucidate a new problem in seismology, and to note the persistent ingenuity with which he followed up an almost invisible line of research.

Between 1880 and 1890 Seismology as a distinct branch of science was in the making, not merely in Japan, but in

Japan for the whole world. The sharp earthquake of February 22, 1880, which did a considerable amount of damage in Yokohama and Tokyo, had one important scientific consequence. It led to the realization of an idea which had for some time been hovering in the minds of Professors Milne, Chaplin, Ewing and others, namely, the establishing of a society for the study of earthquake phenomena, to be called the Seismological Society of Japan. With a membership of over one hundred, of whom about one-third were Japanese of position and influence in educational circles, this Society entered upon a brief but vigorous life of twelve years' duration. The papers read at its meetings and published in its Transactions did more to give shape to the modern science of seismology than the work of any other institution or combination of institutions the wide world over. The most conspicuous of this truly cosmopolitan band of enthusiasts was unquestionably Professor Milne, who, in addition to enriching the Transactions by his numerous and pregnant contributions, sent annually to the British Association a report containing the cream of the lively discussions and valuable work done by the members of the Society.

One of my aims has been to show what a remarkable lead the Seismological Society of Japan took, not only in following up the problems originally associated with the names of Mallet, Hopkins, and Perrey, but in striking out on quite new paths. Having done its pioneer work the Society came to a natural end in 1892. The study of seismology had then become of national importance, and was receiving strong support from the Imperial funds under the enlightened rule of the Emperor of Japan.

During the existence of the Seismological Society sixteen volumes of Transactions were published; but the publication was continued by Professor Milne till 1895 under the

title of the *Seismological Journal of Japan*, the four volumes of which were regarded as corresponding to volumes XVII to XX of the Society's Transactions. Complete sets of these important publications are difficult to obtain, many copies having been destroyed in 1895 by a fire which practically consumed Professor Milne's own precious library of seismological literature.

In no respect do I consider this book to be a complete account of earthquake phenomena. For these the reader must refer to Professor Milne's own publications—*Earthquakes* and *Seismology*; or to Montessus de Ballore's large volumes —*Les Tremblements de Terre* and *La Science Seismologique.* I have purposely limited my discussion to those phenomena which have suggested physical investigations, or which from their nature touch closely on physical theory. In short, I treat the subject not as a branch of technical geology, but as belonging to the wider domain of natural philosophy, both experimental and mathematical. Consequently there are important aspects of seismology which are passed over in silence or are referred to only incidentally.

I have culled largely from the writings of recent investigators, reproducing in many cases their diagrams and illustrations. Some of these diagrams have been considerably reduced, so as to obviate the necessity of using folding plates. In no case, however, so far as I am aware, have the broad features of the diagrams under consideration been obliterated. While thanking all from whom I have benefited in this way, I would specially record my indebtedness to Prince Galitzin, Professor Milne, Mr. Oldham, Captain Dutton, Dr. Davison, Mr. Middlemiss, Mr. Heath, the Directors of the Coats' Observatory (Paisley), and last, but not least, to my former pupil, Professor Omori, of the Imperial University of Tokyo. Although differing from the Japanese seismo-

logist on several points, I appreciate and admire to the full his zeal and skill in handling the ever-accumulating statistics and in analysing the intricate records of earthquakes.

During the printing of the book new and better data have come to hand, and important improvements have been effected in the construction of recording apparatus. It was impossible under the circumstances to take account of all of these. The broad principles of the construction and action of seismographs remain the same; but, as in every other branch of science, improvements must come. All honour to him through whom they do come. In particular, Professor Wiechert, of Göttingen, has in recent years effected notable developments; and in Professor Marvin, of Washington, we have the promise of a seismologist of singular clearness of vision.

CARGILL GILSTON KNOTT.

UNIVERSITY OF EDINBURGH,
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# CHAPTER I

## INTRODUCTION

Astronomy and Geology. The Folded Rock. Stresses and Strains. Bending Yielding Rupture. Flow of Solids—Adams and Nicolson. The Crust of the Earth. Isostasy. Elastic and Quasi-elastic.

IT is worthy of remark that the oldest science is Astronomy and the youngest Geology. Ages before any systematic attempt had been made to unravel the complexities of the structure of the earth on which we live, the human mind had grasped the scientific unity of the Cosmos.

The reason of this is not far to seek. It lies in the apparent simplicity of the celestial problem, which early disclosed itself to the gropings after knowledge of the ancient Chaldeans, Akkadians, and Indians.

The foundation was laid by the astrologer, who from the ordered movements of the heavenly host would fain read the hidden scroll of futurity. Impelled by the eager chase after things occult, the mind of civilized man gradually accumulated a mass of knowledge, the mere knowing of which became in time its own reward.

The early crystallization of this mass of knowledge into a body of scientific truth depended in large measure upon the comparative simplicity of the problem of the solar system. The modern astronomer, aided by his instruments of research, knows of complexities in the heavens far surpassing anything presented by our own system of sun and planets. If, instead of having one great central orb greatly exceeding in mass all the planets and satellites combined, we had been blessed with what is known as a double star system, then truly would the ancient astronomers have been sorely tried in their efforts to reduce the motions of suns and planets to combinations of cycles. In the face of such a stupendous problem the mathematical powers of our race might possibly have developed more rapidly than has been the case. But it is infinitely more probable that, with two suns instead of

one influencing the motion of our earth, man would have been content to accept the facts without making any effort to find a formula to co-ordinate them. The very complexity of the problem would have led to its neglect; and so has it been with geology.

To form a theory of astronomical phenomena needed only a certain basis of geometry and arithmetic. Guided by the broad and therefore simple facts of experience, man soon recognized a rhythm in celestial motions, and by ingenious use of the geometry of the circle was able to express this rhythm quantitatively with an accuracy sufficient for his wants. But in the chaos of rocks and hills, in the apparently lawless recurrence of earthquakes and storms, he found no guiding line.

Even after the Newtonian system of worlds had been established as the first great scientific generalization, man's knowledge of his own planet remained practically nil. Some adventurous minds had speculated on the causes of earthquakes and volcanoes, and even on the interior constitution of the earth. But not till the nineteenth century did geology as a science begin to take form.

Looking back from the scientific height now gained, we easily see why geology should be among the most recent of sciences. It requires for its true development a sound knowledge of physical principles, chemical facts, and anatomical structure. For instance, how can we hope to read the page of the folded rock unless we know to some extent the effects of heat and of stress and strain upon heterogeneous material.

*A folded rock.* The very words suggest a past—suggest a first state of unfoldedness, a simple stretch of pebble and sand laid down along the edge of a continent or island by the never-ceasing action of running water and the ebb and flow of tide. The ocean rises or the land sinks; and the sandy, pebbly stretch becomes covered with newer sediment, and finally, by pressure and cohesion, it is consolidated into a rock. Sandwiched in between layers of older and of newer age, this particular stratum enters upon a life history of extraordinary vicissitudes. It is bent, doubled, folded,

drawn out, snapped across, and tortured in all kinds of ways; and yet, through all, it retains the memory of its birth—it remains a stratified rock. Possibly through the influence of heat or pressure it may have lost its more obvious stratified character or assumed a false stratification; but its origin is clear as noonday to the experienced eye of the geologist.

Moreover, it is because of its stratified character that we know it to have been the victim of these varied strains of bending, doubling, and folding. Had the solid crust of our earth been isotropic, having equal properties in all directions, we should never have been able to recognize those foldings and bendings of rock, or to infer the existence of the enormous pressures, pulls and thrusts, which must have accompanied, and in a sense, produced the strains. But by the very mode of its formation under the combined influence of hydrodynamic and gravitational forces our stratified rock is aeolotropic, and preserves its characteristic aeolotropy through aeons of change. Thus the pure surface action of air and water is an indispensable factor in enabling us to determine the so-called hypogenic forces which also help in the process of earth sculpture.

There are other factors, such as volcanoes and geysers, whose evidence is very direct as to the igneous or plutonic character of the hidden places of the earth. It requires no technical knowledge of physical principles to infer from such phenomena that the interior of the earth is at a high temperature. But it is quite otherwise when we come to consider the scientific evidence based on the convolutions and fractures of stratified or sedimentary rocks like conglomerates, sandstones, and shales. From the date of their deposition in horizontal or nearly horizontal layers, along the margins of seas or lakes, they have passed through a varied experience. They have sunk to form the floor of lakes and seas, and have then been raised into plateaux and mountain masses. This process of alternate upheaval and subsidence may have been repeated many times, slowly no doubt, but none the less surely. And just as an ocean swell in its majestic motion means at every point a rise and

fall, and over each bit of surface a curving and an uncurving, so has it been with the see-saw motion of the earth's rocky crust. Slow but irresistible, with prolonged pauses, these movements of the crust as demonstrated by the succession of strata were of necessity accompanied by flexure of rock stretches originally flat. We can, in the geological record, find examples of all kinds of flexure, from a single slight convexity or concavity to great synclinal and anticlinal folds, such as constitute the Jura mountains, the Carpathians, or the Appalachians.

Let us consider for a moment the nature of the system of forces which might be imagined as giving rise to this condition of things. If we assume that the strata have not been appreciably extended along the direction of their original stretch, then we must admit a great pressure compressing their whole length into a distance smaller than the original length. The source of this tangential pressure has generally been referred to the contraction of the earth as it cooled. Experiments by Favre, Daubrée, Mellard Reade, Willis, Cadell, and others, on the effects of lateral crushing on the form of layers of wax or clay have certainly gone far to prove the sufficiency of this explanation. Some may at first find it difficult to imagine how a solid stratum of sandstone can yield by bending and not be ruptured in the process. But it has been established experimentally that a solid will flow like a very sluggish liquid when a suitable combination of forces acts upon it.

Of the many interesting experiments bearing on this subject, perhaps the most important from our present point of view are those conducted by Professor F. D. Adams and Dr. J. T. Nicolson on the flow of marble.[1]

Small columns of pure Carrara marble were surrounded by close fitting iron tubes and then subjected to end pressures. When the pressure reached about 18,000 lb. to the square inch the tube surrounding the marble began to bulge, and on removal of the pressure the marble was found to be permanently distorted. The deformed marble was firm and

[1] *Philosophical Transactions*, vol. cxcv, *A*, 1901; or *Proceedings of the Royal Society*, vol. lxvii, 1900.

compact, but it could be distinguished at once from the original unstrained rock by its turbid appearance. Microscopic examination showed that the material had broken down along certain lines of shearing, the granulated minutely broken structure being what is known as cataclastic. In this condition the rock was found to have become markedly weaker than at first. When, however, the same treatment was given to the material at a high temperature of 300° and 400° C., no cataclastic structure could be detected, and the resistance to crushing had not appreciably diminished. In this case there was no breaking down, and the whole movement was due to the changes in the shape of the component calcite crystals.

Thus under suitable stresses cold rock can be permanently strained, becoming moulded to new forms by breaking up along the lines of greatest tangential stress, but yet without disintegration. The rock becomes less resistant during the process, and subsequent straining is more easily effected.

Bearing these facts in mind, we may imagine the bending and folding of strata to take place in the following manner. The strata will begin to yield where the appropriate combinations of pressure and tension are most developed. As soon as a particular part has yielded sufficiently there will be relief of stress, and the yielding will cease. The condition of stress favourable to further yielding will probably be developed somewhere else along the stratum, which may in this way become gradually bent and folded more or less along its whole length.

If during this process fracture occurs, immediately an earthquake shock will be transmitted through the neighbouring material. If the fracture be accompanied by a relative displacement of the fractured faces—in other words, if a fault be produced, the rocks in the vicinity will experience a seismic disturbance, which will be transmitted to distances varying with the strength of the disturbance.

Faults or dislocations in the continuity of the strata which compose the earth's crust are the rule and not the exception, and imply the ceaseless action of powerful pressures and tensions in suitable combination. These must result from

the action of some general terrestrial cause, such as we may associate with the cooling and shrinking of the earth. The most probable combination of stresses on the assumption of a cooling and contracting earth will be tangential or horizontal pressures with possibly tensions acting vertically. The resulting lines of maximum shearing stress will then be inclined at angles of 45° to the horizontal; and in directions approximating to these we should expect the yielding to take place. This matter is discussed in an interesting manner by E. M. Anderson, of the Scottish Geological Survey, in a paper on the Dynamics of Faulting.[1]

It is thus abundantly evident that the crust of the earth has been subject, and still is subject, to the influence of forces and stresses, the ultimate source of which is gravitation acting on a heterogeneous mass.

When using the phrase 'crust of the earth' we must not regard it as being more than a convenient term for the superficial layers more or less closely resembling the parts which are accessible to direct observation. It is no doubt a survival of the crude hypothesis that the earth is molten inside and covered with a comparatively thin shell of solid matter; but this hypothesis is no longer tenable.

So far as it is accessible to us the crust of the earth is intensely heterogeneous in its structure; and we may assume a corresponding heterogeneity down to a depth of, say, ten to fifty miles.

Such fairly deep-seated heterogeneity seems to be necessary when we consider the significance of continental raised areas and oceanic deeps. From the highest crests of land to the deepest caves of ocean there is a drop of, roughly, twelve miles. Were the material which builds the Himalaya clump of the same density as that which forms the floor of the ocean, a system of stresses would be called into existence which would cause a real flow of solid matter from beneath the continental dome. We seem rather to be compelled to assume that the average density of mountain building rock is less than the density of the material which lies beneath the ocean waters. In this way a certain average state of

[1] See *Transactions of the Edinburgh Geological Society*, vol. viii, 1905.

equilibrium is preserved in the crust of the earth, a so-called isostatic state or state of isostasy. The condition may be simply illustrated by means of the hydrostatic experiment of the U-tube with mercury in the bend when water is poured into the one limb. The water surface in this limb is at a much higher level than the mercury surface in the other, and yet the conditions of hydrostatic equilibrium are satisfied through the mercury substratum.

It is admitted generally by geologists that continental areas have increased, and that seas have deepened during the progress of geologic ages. One strong piece of evidence in favour of this view is the wider geographical distribution of similar animal types the further back we go in geological time. If we accept this conclusion, one immediate consequence is that on the whole the conditions which make for upheaval must have over-balanced the conditions which make for denudation and waste. The isostatic state is therefore never one of complete equilibrium, but only an approximation to it. Readjustments are always going on as the mountain crests and slopes are worn away; and it is this process of continual readjustment which every now and again culminates in a sudden fracture or snap, and generates an earthquake.

The probable location of all earthquakes is therefore in the heterogeneous crust. It has been, and still is, the seat of extensive changes; and earthquakes are the necessary concomitants.

Let us suppose, then, that some disturbance takes place in the earth's crust—a faulting or slip of contiguous rocks, a snap or explosion or a cave-in of material—anything, in fact, which involves a transmission of shock outwards. What kinds of motion would be naturally expected?

1. With sufficiently intense shock and appropriate combinations of stresses the material might be fractured or disintegrated.

2. With less intensity of shock there might be straining which just stopped short of disintegration or fracture, but which left a permanent change of configuration of neighbouring parts.

3. There might be merely temporary changes of configuration, from which the material recovered after cessation of the shock.

All these kinds of motion are met with in earthquakes. The first class necessarily involves the second and third; and the second involves the third. It is not possible for a fracture or rupture to occur without giving rise to displacements and vibrations of less violent character from those which just stop short of breakage down through an infinity of gradations to those which are true elastic vibrations. The classification given above is not perhaps severely scientific—no classification ever is; but it is convenient for purposes of discussion.

There have been, and are, dislocations in rocks intermediate in character between the clear rupture and the permanent shearing without rupture. Nevertheless, broadly speaking, there is a marked distinction between these two kinds of strains. As soon as a crack or break occurs the molecular conditions are abruptly changed, and the physical problem is altered. At the instant of occurrence of the break, unbalanced molecular forces come into existence; and these produce in the material on both sides a dynamic shock which is transmitted in all directions as an elastic or quasi-elastic disturbance. These terms, elastic and quasi-elastic, will be found very convenient, and indeed almost necessary, when reference has to be made to the kinds of non-rupture movements just described.

Elastic is used in its true physical sense of that property in virtue of which a substance resists deforming or compressing stresses, and recovers its original unstrained condition when the stresses cease to act. It must not be confused with such properties as extensibility or flexibility. A glass fibre is much more flexible than a rod of the same material; but their elasticity is the same.

So long as the stresses and accompanying strains are not too large, solid substances behave with fairly perfect elasticity; but beyond certain limits, which experience alone can determine in any particular case, there is nothing like complete recovery when the stresses are removed. Under

these conditions we may speak of the substance as being quasi-elastic. A simple but instructive illustration of the distinction between perfect elasticity and quasi-elasticity is afforded by the behaviour of india-rubber under tension. This substance, known as elastic in some of its forms, is not really more elastic, and, indeed, is in a certain sense less elastic, than steel. When the load is gradually increased the stretched rubber does not immediately attain its greatest extension under a given value of load. When left for some minutes it gradually grows in length; and, similarly, when the load is removed, after it has acted for some time, the rubber does not immediately recover its original unstretched length. Gradually, however, it will creep back towards this original length, reaching it after a prolonged interval of time. Similar effects are produced when rocks are subjected to stresses of various kinds.

It is clear, then, that seismological phenomena, dealing as they do with changes of form of rocks under great stresses, demand for their elucidation some acquaintance with the facts and theories of elasticity. The more logical course would be to enter upon a preliminary discussion of these facts and theories. Since, however, the more important applications of the theory of elasticity are required only when the modes of transmission of earthquake tremors come to be considered, we shall find it more convenient to defer discussion of this theory until after the more outstanding phenomena of earthquakes have been described. To the description of these phenomena we now proceed.

# CHAPTER II

## EARTHQUAKE PHENOMENA

Immediate and Transient Effects of an Earthquake. Earthquake Sounds. Stifling Effect of Viscosity. Destructive Effects. Undulations. Projection of Stones. Buildings, Tombstones, Monuments. Vorticity of Ground. Horizontal and Vertical Displacements. Contraction and Expansion of Areas. Cadastral Surveys.

In this chapter we shall first consider the immediate and usually transient effects of earthquakes as they appeal to the senses of the inhabitants of the districts visited; and then the conclusions which may be derived from a study of the permanent effects as illustrated chiefly by damage to works of human construction, and by changes of configuration on land and water.

It will serve no scientific purpose to describe in rhetorical language the earthquake in all its horrors. Words can give no adequate idea of the sensations experienced and emotions evoked by a quake, even when it is slight and unaccompanied by death and destruction. When the shock is severe, causing havoc and dismay, the experiences of the unfortunate victims beggar all description.

Probably the most evil feature of the earthquake is its suddenness. It is true that in the vast majority of cases a severe shock is heralded by a series of preliminary shocks of slight intensity. These might be taken as forewarnings, if it were not that they occur as a rule in seismically sensitive districts where slight shocks frequently happen without leading up to a large earthquake. It can hardly be said that familiarity breeds contempt; yet the very frequency of these light shocks in countries like Italy and Japan disarms the inhabitants of any immediate apprehension of a seismic catastrophe. Only after the havoc has been wrought does the memory recall the sinister warnings of hypogene action.

A destructive earthquake is always accompanied by sounds.

Sounds are also frequently, though not always, heard with moderate shocks which last a few seconds and do little or no damage. The great majority of weak shocks, and even many fairly strong ones, are unaccompanied by any (audible) sound; on the other hand, earthquake sounds may be heard yet no shock be felt. Sometimes the sound precedes, sometimes succeeds, the shock proper; at other times shock and sound are practically simultaneous. The audibility of the earthquake sound will depend no doubt upon the powers of audition of the residents in the district visited, but will also be conditioned by other factors of a more or less accidental nature. Of these we may mention the quiet or bustle of the neighbourhood, the hour of night or day, the character of the soil or rock in the immediate vicinity. The sensation of sound is produced by any sufficiently rapid and sufficiently large variation of pressure in the air; and if the sound is fairly continuous with an approach to definiteness in pitch, the variations of pressure must be approximately periodic with a frequency exceeding thirty or forty vibrations per second. The disturbance in the air need not be great provided its periodicity is sufficiently pronounced.

The sounds heard during an earthquake have been variously described—the commonest description being, what I have myself experienced, that they are like the rumbling of a vehicle. When feeble, they have more of a booming character, and when very strong they suggest thunder or the rattle of musketry.

Whatever be the original character of the disturbance in the earth, it must first pass into the air as a compressional dilatational disturbance capable of affecting our ears. In chapter x, below, it is shown that a very small, perhaps not a thousandth part of the energy of wave motion in the rock can pass by refraction into the air. Also since the speed of propagation of the compressional wave in air is much smaller than the speeds of propagation of elastic waves in rock, the direction of propagation of the wave in air must be nearly vertical, however oblique to the surface the elastic wave in the rock may come. This at once explains why the sounds always seem to come from the ground.

If we exclude for the moment the case of severe earthquakes which produce fissures and cracks in the ground, it is evident that the horizontal motion of the ground (except in so far as it sets vertical walls into harmonic oscillations) can have little if any effect in sound production. The compressional wave in air must be almost entirely due to the vertical motion of the ground. In cases in which sound is heard before the shock is felt, the origin of the shock is probably at some distance from the locality, so that time may be given for the comparatively small rapid elastic vibrations to run ahead of the larger quasi-elastic vibrations which constitute the sensible shock. There is no doubt that such rapid vibrations do run ahead of the larger disturbances; and the reason why audible sound does not always precede the shock proper is simply because the viscosity of the material stifles the vibrations of short periodicity. The nature of the soil and underlying rock at a given locality have, indeed, a distinct influence on the sound phenomena. Hard rocky soil is favourable to the production of sound; while soft alluvial soil is comparatively unfavourable.

The main factor in the production of earthquake sounds must be the intensity of the shock itself. The greater the intensity the greater the chance of sounds being heard. Dr. Charles Davison[1] has given an admirable account of many of the sound phenomena accompanying earthquakes. In particular he discusses the data provided in Milne's great catalogue of 8,255 Japanese earthquakes, and finds (as was of course to be expected) that the percentage number of earthquakes accompanied by sounds increases with the disturbed area, that is, with the intensity. As the disturbed area increased from 100 square miles to 10,000 square miles the proportion of shocks accompanied by sounds increased from 12 to 70 per cent. It is obvious that, *other things being equal*, the more widely distributed the earthquake the more chance there will be of sounds being heard, not only because of the greater number of people subjected to its influence, but also because it is itself a more powerful shock. Davison

[1] *Philosophical Magazine* for January, 1900.

is indeed of opinion that 70 per cent. is a low percentage for quakes disturbing 10,000 square miles of surface. This low percentage he regards as evidence that the Japanese as a people are defective in the power of hearing low sounds.

In the same paper Davison points out that the comparatively weak shocks which visit Great Britain are almost always heard as well at felt—indeed sometimes heard only; whereas in Italy there is not the same high percentage of audibility even with much stronger shocks. It is interesting also to note that the Charleston earthquake of 1886 was felt by many who heard no sound; and this comparative freedom from sound was not confined to places remote from the epicentre.

The whole evidence indicates that several factors enter into the question. One of these no doubt is the personal equation of the observer and recorder. But the italicized statement a few sentences back, namely, *other things being equal*, is a necessary qualification in all cases of comparison. An earthquake, deep-seated in its origin, may be felt over a much larger region of the earth's surface than one originating at a much less depth. But it is highly probable that the rapid vibrations which are essential for the production of sound may be more completely stifled by the viscosity of the material as they pass through a greater thickness of the earth's crust. Hence a wide-spread earthquake, coming from a deep-seated source, may produce no sound phenomena in places where it is generally felt, whereas a comparatively feeble shock of limited extent and shallow focus may be heard even where it is not felt.

During my eight years' residence in Tokyo, Japan, I felt many earthquakes of the moderate intensity which just stopped short of doing damage; but those which were accompanied by a rumbling sound distinct from creaking of walls and rattling of ornaments were comparatively few. Other Europeans resident in Tokyo and Yokohama had the same experience. Now, in any general catalogue of Japanese earthquakes, those which are felt in the Tokyo-Yokohama region constitute a large percentage and cannot fail to impart to the whole any marked characteristic peculiar to

them only. The plain of Musashi is a great stretch of alluvial soil, just the kind of material fitted to kill out rapid vibratory motion. This consideration seems to be a sufficient explanation of the comparative scarcity of Sound phenomena in Japan, without the assumption of any general acoustic deficiency among the Japanese, or among European residents in Japan.

In his account of the Assam earthquake of 1897, Oldham mentions that the earthquake was heard, not felt, in the mines of the Raniganj coal-fields, although at the surface above the shock was universally felt and caused some damage to buildings. This is an interesting illustration of the fact that the surface movements due to an earthquake are greater than the movements underground. The vibrations of the walls and bottom of the mines were sufficient to produce sound waves in the air columns filling the mines, but were not great enough to cause motions perceptible to the other senses.

It is obvious on general grounds that viscosity must tend to kill out the rapid vibrations; and Nagaoka's and Kusakabe's experiments (see below, chapter ix) on the lack of perfect elasticity displayed by rocks are of importance in this connexion. It has been long known that, when imperfectly elastic materials are strained under stress gradually applied and then as gradually removed, the measured strain corresponding to any particular value of stress has a different value according as this stress is approached from lower or higher values. When we subject the material to a cycle of stresses between given limits the graphical representation showing the relation between strain and stress consists of two distinct curves enclosing an area. This area is the measure of the energy lost because of the viscosity of material. Now Kusakabe has found that this area is distinctly greater in some rocks than in others. A few of his curves are shown in the figure. Stress is measured horizontally and the corresponding strain vertically. The greater the area of the loop, the greater the loss of energy due to viscosity. The general result indicates that the older rocks are less viscous than the more recent.

It is clear, then, that the elastic character of the rock through which earthquake vibrations are being transmitted

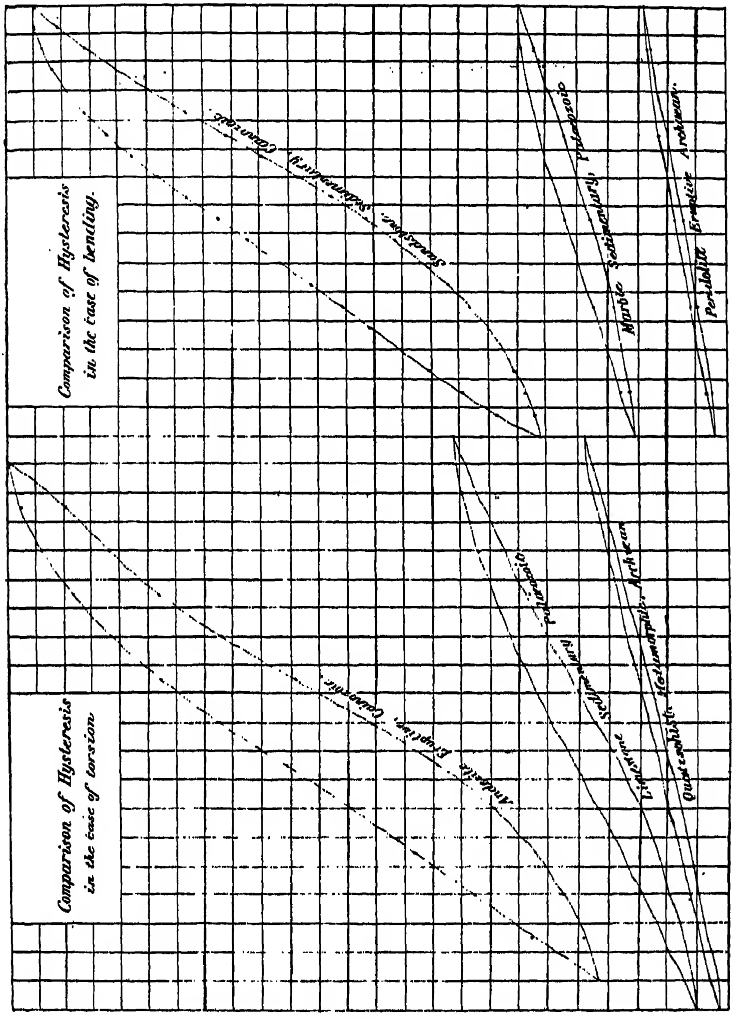



FIG. 1.

must affect the nature of the motions felt and the sounds heard. Kusakabe[1] has discussed from this point of view

[1] Frequency of After-Shocks and Space-Distribution of Seismic Waves. By S. Kusakabe. *Journal of the College of Science, Imperial University of Tokyo* (vol. xxi, 1906).

the relative frequencies of earthquakes in the centre of Japan, and has found evidence of a direct relation between the frequencies in contiguous districts and the geological nature of the rocks in these districts.

I propose now to pass in rapid review some of the more striking phenomena which characterize destructive earthquakes within the region known as the epicentral tract. Here buildings and monuments are overthrown and damaged, pillars are rotated, the soil is cracked and fissured, rivers are dammed up and lakes are drained, fields are so altered as to necessitate re-surveying; in short, every possible change of configuration of the surface of the land may occur as an accompaniment of an earthquake.

Since Mallet made his classical Reports (1850–61) on the Great Neapolitan Earthquake, the varied permanent after-effects of every large earthquake have been carefully examined and recorded by competent observers. From these Reports much scientific knowledge has been gained, although not perhaps of the kind most desired. At first statement of the problem it might seem to be a simple enough matter to gather, from the nature of the damage done, some fairly definite idea of the direction and strength of the shock. But the universal experience is quite otherwise. The reasons for this will be apparent as we proceed.

The wave-like motion of the ground during an earthquake has been often described by observers. Take for example the following accounts by Dr. Parker and Mr. Blackman as given by Major Dutton in his report[1] of the Charleston shock of 1886. Dr. Parker says, 'The vibrations increased rapidly and the ground began to undulate like a sea. The street was well lighted, . . . and I could see the earth-waves as they passed as distinctly as I have a thousand times seen the waves roll along Sullivan's Island beach. . . . I could see perfectly and made careful observations, and I estimate that the waves were at least two feet in height . . . . I saw a brick wall . . . reeling from west to east, and am sure that it leaned over at times as much as forty to forty-five degrees from the perpendicular.' Mr. Blackman set himself

[1] *United States Geological Survey*, Ninth Annual Report, 1887–8.

deliberately to study the phenomenon 'at all hazards', and reported as follows: 'After the first vertical tremor had passed, and while I was being swayed to and fro by the succeeding horizontal movement, I distinctly saw four or five separate waves pass across Tradd Street from the north-east to the south-west. As nearly as I can estimate the width of the several waves, they were about as wide as the roadway between the sidewalks; as to their height, I would not like to venture an estimate, but each seemed to be at least a foot high.'

Estimation of wave height in such a case must be extremely difficult, for there is no fixed vertical line to serve as standard of comparison. The 'two feet in height' is almost certainly overestimated, and probably also the one foot height. But even taking the estimates as they are, we see at once that the waves must be fairly long and flat, and that it is impossible for the slope of the ground to attain anything like an angle of 45°. In forming an estimate of the deviation of a wall from the vertical, the observer has the same difficulty as that already mentioned, the absence of a steady vertical line to serve as guide. There is also his own rocking motion to be taken into account, as well as the motions of surrounding bodies. A particular combination of these various rocking motions might quite easily exaggerate the apparent motion of a particular wall. There can be little doubt, however, that the ground is thrown into distinct waves, which may travel partly as gravitational, partly as quasi-elastic flexural waves.

The impulsive motion of the ground is proved by the manner in which blocks of stone partially imbedded in the soil are loosened and projected several feet from their original position. Oldham, in his account of the Indian shock of 1897,[1] gives some interesting illustrations of this effect. Stones of various sizes were found displaced along the level through distances which varied as a rule from 2 ft. to 4 ft. There were comparatively few cases of smaller displacements than the lower limit mentioned, showing that

[1] *Memoirs of Geological Survey of India,* vol. xxix.

when the impulse was sufficient to loosen the blocks from their hold of the ground in which they were half imbedded, it was also sufficient to project them through a horizontal range of at least 2 ft. A splinter of granite 3 ft. long, which had been lying flat on the ground, was projected through a horizontal distance of 8·5 ft.; and an upright monolith some 6 ft. high, was shot out of the ground and through the air in such a manner as to fall at a distance of 6 ft., a deep dent in the ground marking the place where the lower end struck. These cases all indicate considerable impulsive action of the shock. The smallest velocity of projection which will enable a projectile to come to earth at a distance of eight feet from the point of projection is 16 ft. per second; and with this combination of initial velocity and range of projection the angle of projection would be 45°. For small speeds of the kind indicated the resistance of the air is negligible, so that the projected stone would come down to the ground with practically the same velocity as that of projection. But the marks made on the ground by the falling stone seem to require a higher velocity than 16 ft. per second. This would imply either a considerably smaller or a considerably higher angle of projection. Oldham is of opinion that the whole nature of the effect indicates bodily displacement of the ground in which the block was partly imbedded. Any attempt to explain it in terms of the elastic vibrations of the material as the shock impinges internally on the surface of the ground leads to values of accelerations and elastic displacements which seem to be altogether out of the question. The evidence for the existence of surface undulations comparable to ocean swells has been considered and seems to be incontrovertible. The vertical motion of the ground when such waves pass along might easily enough be sufficient to project blocks of suitable size and mass with velocities exceeding the limits given above. It is interesting to note that the blocks which were projected were all, within certain assignable limits, of a particular average size. When the stone is too small the cohesive forces, which depend on the surface of contact of the stone and the soil, are proportionately great in

Fig. 2. Inglis's Monument at Chhatak.

comparison with the momentum communicated to the stone when the ground has its maximum motion ; for this momentum depends on the mass, that is, the bulk of the stone. For larger sized stones the cohesive forces, though greater absolutely, are less effective in comparison with the momentum communicated; hence a greater chance of these becoming loosened and projected through the air. On the other hand, when the stone becomes too large, the greater mass for the same communicated momentum implies a smaller velocity, and projection against the force of gravity becomes impossible.

The effects of earthquake shocks on buildings, tombstones, and monuments have naturally attracted a great deal of attention on the part of seismologists. It is by careful study of these cases of damage or destruction that we gain knowledge as to the best methods of construction in earthquake countries. Also from a purely scientific point of view we gain information regarding the nature and power of the seismic motion itself. With so many typical examples in the great earthquakes of Charleston, Japan, and India, it is not easy to make a choice. Certain cases described by Oldham are, however, particularly instructive. We cannot do better than quote his descriptions and reproduce some of his diagrams.

'The most imposing and striking of the numerous instances of twisting is that of the monument to George Inglis, erected 1850, at Chhatak. This conspicuous landmark takes the form of an obelisk, and rising from a base 12 ft. square, must have been over 60 ft. high before the earthquake. . . . The topmost 6 ft. 2 in. was broken off and fell to the south, while the next 9 ft. was thrown to the east, as shown in plan (in Fig. 3). Of the remainder, the top 22 ft. has been separated at a height of about 23 ft. from the ground, and twisted negatively through 30°.'

The general appearance of this obelisk after the earthquake is shown in Fig. 2, reproduced from Oldham's photograph.

In the figure showing the Inglis monument in plan is shown also the case of two neighbouring pillars which suffered different rotations, the aqueduct pillar being twisted

negatively 5°, and the pillar on the wall 4° in the opposite direction.

In Mr. Latouche's Report, which forms Appendix A to Oldham's memoir, some remarkable cases of damage to tombstones are recorded. One case was that of a small marble pedestal supporting a cross. The pedestal was twisted round through 26°, while the cross fell at the foot.

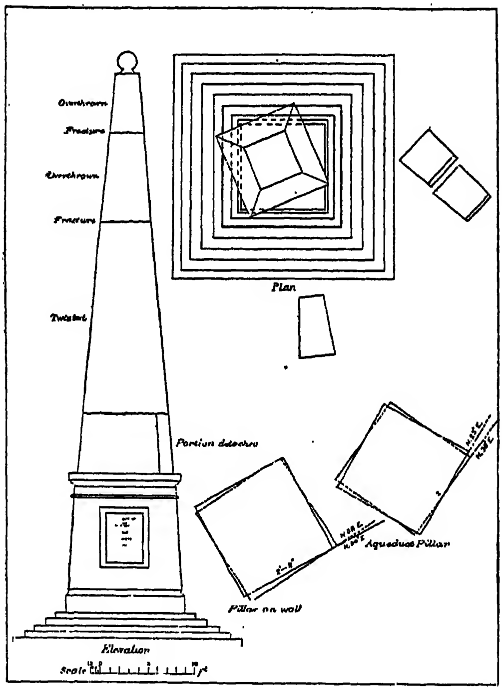



FIG. 3. INGLIS'S MONUMENT AT CHHATAK IN ELEVATION AND PLAN, AND TWISTED PILLARS AT CHERRAPUNJI.

These examples of rotatory effects demonstrate completely the great complexity of the movement of the ground. Two pillars a few feet apart have been rotated in opposite directions and have been displaced in directions almost perpendicular to each other.

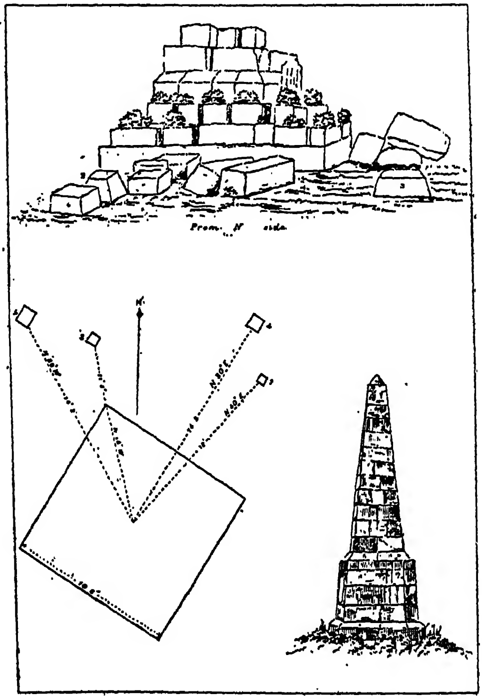
This vorticose movement, which at first sight seems to be

proved by such effects, has not been accepted by some authorities. As is well known in dynamics a simple impulse or blow acting on a body will cause that body to rotate if the impulse does not act through the centre of mass. Hence a succession of blows in varying directions may easily cause a rotation in a body resting on but not imbedded in the soil. But it is difficult to escape from the belief that rotation of an object like a pillar or tombstone which is imbedded in the soil is an effect shared by the ground itself. Indeed how can we have in the ground a succession of blows varying rapidly in direction without something of the nature of whirling or vorticose motion accompanying it ?

It is a familiar principle in the motion of a rigid body that any displacement can be effected, in an infinite number of ways, by means of a translation in a definite direction in combination with a rotation about a definite axis. But it does not follow that this has happened dynamically. In all probability the successive stages through which the highest remaining block in Inglis's Memorial passed formed a complicated sequence of yieldings to successive shocks varying in direction and magnitude.

We may easily imagine a shock of a sudden impulsive nature snapping a pillar at some little height and giving it a slight tilt on one corner or edge, while at the same time the lower part moving with the ground will be both tilted and rotated in a manner not generally similar to that in which the upper part has been moved. The tilt of the upper part is due to the impulsive force acting on it; but the tilt of the lower part is due to its being fixed to the ground. We cannot hope to formulate the complex dynamics of this problem. A general statement is all we can venture on. The rotation and tilting of the support will put the two parts out of relation, so that a perfect readjustment is practically impossible. The chances are that the upper part will fall in a direction and with a rotation determined by such a complexity of forces as to be practically arbitrary. On rare occasions, as in the case of the Inglis's Monument, the upper part will remain poised in position, but permanently displaced from true adjustment.

Another illustration from Oldham's Report may be given as showing the different directions in which different parts may be projected. This is the case of Willan's Memorial at Shillong, reproduced in Fig. 4. Mr. Latouche, who reported this case and supplied the sketch, was able to identify four of the fallen blocks before they had been moved. These



Plan showing position of 2nd, 3rd, 4th, and 5th blocks from top of obelisk.

FIG. 4. WILLAN'S MEMORIAL, SHILLONG.

seem to have been projected one after the other in different directions practically without rotation. It is mentioned that the blocks left on the top of the portion of the pedestal still standing are twisted slightly towards the east. These are only a few out of many similar cases which have been described by geologists and seismologists.

To sum up. There can be little doubt that the earthquake

consists at any one place of different trains of waves passing in various directions, the result of internal reflexions and refractions of the original complex disturbance.[1] These trains of waves will interfere somewhat like trains of ripples on a lake, but in a vastly more complicated fashion. The resultant effect will in many cases change fundamentally in a stretch of a few yards. Here the impulses and accompanying ground movements will give all the characteristics of a whirl or vortex: there they will produce practically pure translational effects unaccompanied by any marked rotational phenomena. The necessary complexity of the motion seems to be sufficient to explain all the observed phenomena. The final effect upon a pillar or wall or house must depend (1) upon the space and time averages of the acting impulses, (2) upon the way in which these vary and especially upon the accelerations.

In the Assam quake of 1897, and in the Japanese disaster of 1891, striking examples were given of the permanent change of areas. The rails of railway tracks, originally straight, were distorted into great serpentine folds, demonstrating that the soil had contracted. Opposing piers of bridges were brought closer at the base while the upper portions were kept apart by the resistance to buckling presented by the bridge. The same contraction was indicated by the buckling of wooden bridges crossing streams or swamps. In other cases the piers were drawn apart and the bridge collapsed.

The displacement of neighbouring plots of ground is beautifully illustrated by a case described by Koto in his memoir on the geological phenomena of the Mino-Owari earthquake.[2] Two trees in a garden originally facing each other in an east-west line were shifted so as to lie in a north-south line. The portions of ground on which the trees stood had been shifted relatively north-west and south-east; and yet in this particular place Koto was unable to see any line of fault. The neighbouring portions of ground had

[1] See below, chapters ix and x.

[2] *Journal of the College of Science, Imperial University of Japan*, vol. v, 1893.

been simply sheared past one another without the permanent production of any recognizable fissure or difference of level.

Along the surface lines of fault in the two earthquakes named above there were found many illustrations of change of level. One of the most perfect of these is the Midor Valley in Japan, where a 20 ft. downthrow occurred right across a tea plantation which filled up the broad base of the valley.

In general, a contraction of area in one district means an expansion in a neighbouring locality; and in the great Assam quake of 1897 evidence of wider changes was obtained in the re-triangulation of the district. In the diagram (Fig. 5) a small part of the triangulation is shown connecting the stations Laidera, Mautherrichan, Mosinghi, and Mun; and the following tables show the comparison of the lengths

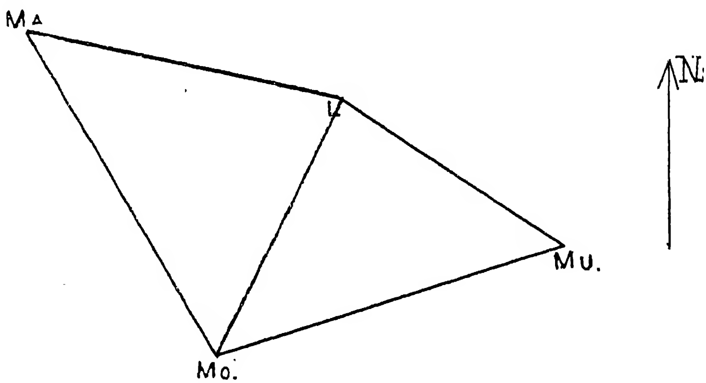



FIG. 5.

and heights of the stations according to the surveys of 1860 and 1898.

| Pairs of Stations | Lengths in feet 1860 | Lengths in feet 1897 | Diff. |
|---|---|---|---|
| *L* — *Ma* | 72,373·1 | 72,377·0 | + 3·9 |
| *L* — *Mo* | 64,350·8 | 64,353·4 | + 2·6 |
| *L* — *MU* | 63,007·7 | 63,007·4 | − 0·3 |
| *Ma* — *Mo* | 83,931·8 | 83,933·0 | + 1·2 |
| *Mo* — *Mu* | 84,893·2 | 84,897·6 | + 4·4 |

| Stations | Heights in feet | | Diff. | Shifts |
|---|---|---|---|---|
| | 1860 | 1897 | | |
| *L* | 6180 | 6186 | + 5 | 2 ft. N. |
| *Ma* | 6288 | 6312 | + 24 | 1 ft. N., 5 ft. W. |
| *Mo* | 5794 | 5798 | + 4 | 3 ft. W. |
| *Mu* | 6212 | 6214 | + 2 | 4 ft. N. |

These numbers are calculated on the assumption that a certain assumed base line had not been altered in length, and a certain assumed station not altered in elevation. These were chosen to the south so as to be as far as possible from the epicentral tract. There is, therefore, a certain doubt as to the absolute values given. Nevertheless they indicate differential movements of the stations too large and too irregular to be attributed to errors of observation.

# CHAPTER III

## SEISMIC SURVEYS

Collection of Reports. Scale of Intensities. Isoseisms. Laibach, Charleston, Assam. Determination of Epicentre. Coseismal Lines. Dutton's Discussion of Time Observations. Determination of Depth of Focus. Dynamical Measure of Intensity. West's Formula. Milne's and Omori's Experiments. Holden-Omori Dynamical Scale of Intensity. Dutton's Method of determining Depth of Focus. Davison's Isacoustic Lines. Twin Earthquakes. Harboe's Herdlinien.

The work of the seismologist begins when the earthquake ends. His duty is to make a careful survey of the district visited by the shock; to collect from those who have experienced it all kinds of information as to movements, sounds, times, durations, and the detailed character of the disturbance; to consider this information as a whole, correcting exaggerations, filling in blanks, and comparing intensities in the various localities from which records have come; to deduce, if possible, the situation and probable depth of the earthquake origin; and finally to draw conclusions as to the nature of the original disturbance.

When the disturbance originates underneath inhabited land, there is generally no difficulty in determining with fair accuracy the part of the earth's surface nearest to the origin —the so-called epicentre, epicentral or epifocal tract. In the case of severe quakes the extent of damage to works of human construction indicates this region with great precision. The method fails more or less when the epicentre lies in an uninhabited or desert region, or even in a district with a scattered semi-civilized population. The seismologist must then make the best he can of purely geological changes such as fissures and cracks.

With moderate earthquakes causing no damage the investigator is compelled to deal simply with the reports of those who felt the shock, and by careful and judicious comparisons endeavour to fix on the region of maximum intensity. It might be supposed that time observations

would prove of value; but practically the great majority of time observations, which are not instrumental, lack definiteness, because of the inaccuracy with which the ordinary individual keeps time.

When the origin of the earthquake is below the bed of the ocean, the difficulty of determining its position is greatly increased. As a matter of fact the greater number of shocks do originate below the sea; and the data at our disposal derived from reports from neighbouring land stations are necessarily incomplete and vague.

Clearly the first desideratum is some method of measuring the intensity of a shock. From a strictly dynamical point of view the intensity of an earthquake, as experienced at any locality, should be determined by the energy of the motion of the ground produced there. But how is this energy to be estimated?

Consider for example the scale of intensities drawn up by Rossi and Forel, and known as the Rossi-Forel Scale. Ten grades are distinguished. For present purposes they may be described briefly as follows:

THE ROSSI-FOREL SCALE OF INTENSITIES.

I. Microseismic shock; instrumentally recorded; perhaps felt by an experienced observer.
II. Extremely feeble: felt by a comparative few at rest.
III. Very feeble: felt by a considerable number at rest.
IV. Feeble: felt by persons in motion, but not generally; disturbance of certain objects.
V. Moderate: felt by every one; disturbance of furniture, &c., quite general.
VI. Fairly strong shock: sleepers awakened; persons sufficiently startled to leave their houses; clocks stopped; oscillation of chandeliers.
VII. Strong: general panic; overthrow of movable objects.
VIII. Very strong: fall of chimneys; cracks in walls.
IX. Extremely strong: partial or total destruction of buildings.
X. Disastrous; fissures in ground; ruin and disaster.

The Mercalli Scale is broadly similar and constructed on much the same lines.

It will be seen that Intensities II to V are determined almost entirely by the sensations of individuals; while

Intensities VII to X are determined by the degree of damage done and the amount of destruction wrought.

Such a scale of intensity cannot of course be regarded as giving a scientific estimate of the energy involved in any particular case ; but it has the great merit of being essentially practical. When the districts experiencing the shock are populous the seismologist has no great difficulty collecting information sufficient to enable him to map out the district

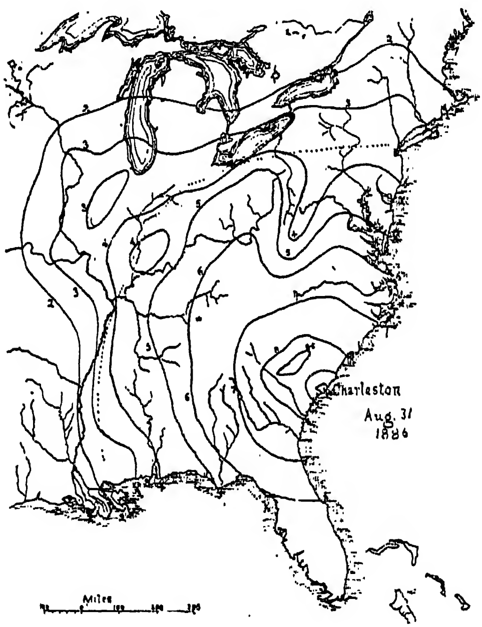
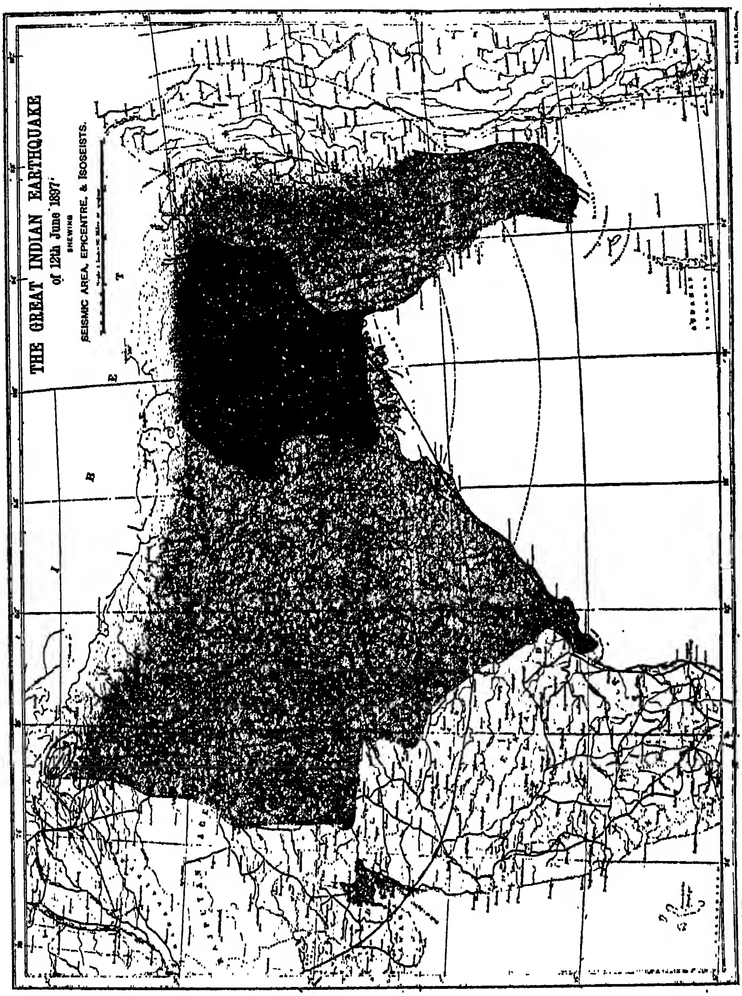



FIG. 6.

by means of 'isoseisms' or lines of equal seismic intensity. Each isoseism passes through localities at which the intensity was the same, and forms a closed loop within which the intensity was in general higher than that associated with the isoseism.

In the accompanying figures two earthquakes are shown treated in this way, namely, the Charleston earthquake of 1886 (after Dutton) and the Indian quake of 1897 (after Oldham). In the former the isoseisms could be drawn only

on the landward side; but there is no doubt that it forms one of the most completely worked out cases of a destructive earthquake. Obviously the isoseism of highest intensity




Area over which the shock is reported to have been felt. Area of extensive damage to masonry buildings. Probable limits of epicentre. The five oval-shaped lines are approximate isoseisms corresponding to intensities X, IX, VIII, VI, IV, on the Rossi-Forel Scale.

FIG. 7.

encloses the smallest area and indicates with fair precision the epicentral region.

In Oldham's chart of the Indian earthquake of 1897

I have added the approximate isoseisms of the earthquake of April 4, 1905. These are shown in the N.W. corner, and the limits within which this shock was felt are indicated by the full line drawn across India and passing near Bombay and Calcutta.

Taking these examples, and also the Laibach earthquake of 1885, very completely worked out by F. E. Suess, let us consider the relation of the intensity on the Rossi-Forel scale to the average distance of the corresponding isoseism from the epicentre. The average distances were estimated by drawing a succession of radiating lines from the epicentre at equal angular intervals, and measuring the distances of the isoseismal lines along these.

| Isoseism R. F. scale | Distance in miles from epicentre at | | |
|---|---|---|---|
| | Laibach | Charleston | Assam |
| II | 201 | 806 | 930 |
| III | | 720 | |
| IV | 142 | 585 | 800 |
| V | 108 | 469 | |
| VI | 75 | 373 | |
| VII | 26 | 239 | |
| VIII | 13 | 136 | 240 |
| IX | 8 | 46 | |
| X | | 13 | 70 |

These numbers, especially in the case of the Charleston earthquake, indicate a fairly good approximation to a true linear relationship between scale intensity and distance from epicentre. If we consider the distances between the same limiting intensities for the three cases, we get as follows:

| Earthquake | Distance between isoseisms | | | | | |
|---|---|---|---|---|---|---|
| | II & IX | Av. | IV & IX | Av. | IV & VII | Av. |
| Laibach | 193 | 28 | 134 | 27 | 116 | 39 |
| Charleston | 760 | 109 | 539 | 108 | 346 | 115 |
| Assam | 730 | 106 | 600 | 120 | | |

It is not possible to give a result for the Assam shock between the limits IV and VII, since the intermediate intensities are not obtainable with sufficient accuracy. Any

attempt to do so would depend on simple interpolation, which virtually assumes what it is desired to investigate.

The columns headed Av. give the average distance between the successive isoseisms. They are inversely as the average intensity gradient between the assigned limits.

In both the Laibach and Charleston quakes there is diminution of intensity gradient between the limiting isoseisms IV and VII. The most interesting feature, however, is the much more rapid fall off of intensity with increase of distance in the case of Laibach than in the other cases. Now when a simple disturbance spreads out in all directions from a particular source, we should not expect that the original intensity of the shock at the source would affect the rate of decay from one lower intensity to another. The severe earthquake will, of course, be felt farther than the moderate shock, because it begins with a higher intensity; but once the intensity at any locality due to the larger shock becomes equal to the intensity that would have been produced by a nearer but smaller shock, we should expect the rate of diminution thereafter to be very similar. And probably this would be so were the shocks dynamically similar in beginning, continuation, and finish. But such similarity between shocks originating in different places can hardly be the rule. It is obvious, for example, that the mere duration of an earthquake shock, quite apart from the energy of motion taken up by the ground during one second, must influence the estimate of the intensity. For the longer the shock lasts the more likely is it to be felt by every one. A prolonged shock, but not very energetic, might be estimated higher in the scale than one of shorter duration but intrinsically more intense. These considerations show how very far short of ideal the Rossi-Forel scale of intensity falls. There is a lack of scientific precision about it. Attempts to obtain a dynamical measure of intensity will be referred to later.

The highest isoseism determines the epicentre; but the exact localization is usually complicated by the fact that the shock has radiated from two or more centres or from a long drawn out source of disturbance. The highest

isoseisms are rarely circular, frequently not even approximately so. In the case of the Indian, or Kangra earthquake of April 4, 1905, the isoseism VIII consists of two detached ovals 50 or 60 miles apart, indicating a twin focus. These may be seen in the north-west of India (Fig. 7, p. 29); also below (Fig. 11, p. 45).

In the case of the many earthquakes which originate under the ocean, the epicentre can be only approximately localized. The configuration of the isoseisms as drawn on the neighbouring land may give some indication, but at the best a very uncertain one in the great majority of cases. The detailed configuration of any isoseismal line depends upon distinctly local conditions, such as the character of the soil and the geological structure of the crust. Thus in certain cases barriers seem to exist which shield particular districts from the disturbance, producing, in short, earthquake shadows.

Much more definite information might be gained from a knowledge of the times at which the earthquake occurred at several widely scattered localities. This method is, however, subject also to considerable uncertainty. First, there is the necessity for *accurate* time-keeping at a sufficient number of well-distributed localities; and, secondly, there is the difficulty of identifying at the several localities a particular phase of the shock. Generally we must be content with the timing of the first impulse or of a marked maximum. Assuming, however, that an accurate timing of a definite phase is possible at several distinct places, let us consider how the information may be used to locate the epicentre.

Let $E$ be the (unknown) epicentre, and $P$ the position on the earth's surface at which the shock is felt at time $t$. If $T$ is the unknown time at which the shock occurs at the epicentre, the difference $(T-t)$ may be regarded to a first approximation as the interval of time taken by the shock to pass from $E$ to $P$. Then $EP/(T-t)$ will be the *apparent* speed of transit of the shock.[1] We may suppose this quantity

[1] For the relation between the apparent speed along the surface and the real speed of propagation, see chapter x, p. 173, and chapter xii on Seismic Radiations.

to be expressed in terms of the data for a number of stations, and to be the same in all directions. We then get a series of equations

$$\frac{EP_1}{T-t_1} = \frac{EP_2}{T-t_2} = \frac{EP_3}{T-t_3} = \frac{EP_4}{T-t_4} = \frac{EP_5}{T-t_5} = \text{etc.}$$

the number of independent equations being one less than the number of stations. But to locate the position of $E$ requires two numbers (latitude and longitude, say), which with the time $T$ make three unknowns. Hence there must be at least four stations, $P_1P_2P_3P_4$, at which the times $t_1t_2t_3t_4$ are known in order that the epicentre may be determined.

The assumptions underlying this method are, strictly speaking, not warranted; and the method could be applied only to cases in which the distances ($EP$) are large compared to the depth of the focus. For it is from the focus and not from the epicentre that the disturbance radiates. The method may be made theoretically more plausible if we take the focus $F$ instead of the epicentre $E$. Then to determine $F$ we need three numbers (latitude, longitude, depth)—hence, including $T$, four in all. In this case we must be given five stations at least.

When the observations are numerous enough, we may form a chart of 'coseismal' lines, each of which is defined as a line passing through places at which some definite assignable phase of the shock was simultaneous. Milne has used the method with some success in determining sub-oceanic origins by means of the coseismal lines of the sea waves; but the inherent difficulties of getting sufficiently accurate time data prevent the method from being a practical one. Attempts have also been made to utilize time observations of the shock proper, as transmitted through the earth, and of the sea wave started in the ocean above the epicentre; and under specially favourable conditions satisfactory results have occasionally been obtained.

There is no reason why, with the installation of suitable recording instruments, the determination of coseismal lines should not be greatly increased in accuracy; but even with

these accurately determined, the localization by their means of sub-oceanic origins must at best be approximate, partly because of the generally elongated and asymmetrical form of the focus itself, and partly because of the different ways in which different rocks respond to the disturbance transmitted through them.

As an illustration of the uncertainty which attaches to time observations we may take Dutton's account in his valuable memoir[1] on the Charleston earthquake. That shock was experienced by a widespread community accustomed to accurate timekeeping; and yet on careful examination of the records we find many discrepancies which prevent a satisfactory delineation of the coseismal lines. For example, there was a marked tendency for observers to reckon by five minute intervals. This is clearly brought out in the following table abridged from Dutton.

| | | | |
|---|---|---|---|
| Number of Reports giving | 9 h. | 50 m. | =32 |
| " " | | 51 | = 6 |
| " " | | 52 | =25 |
| " " | | 53 | =28 |
| " " | | 54 | =31 |
| " " | | 55 | =86 |
| " " | | 56 | =21 |
| " " | | 58 | = 5 |
| " " | | 59 | = 3 |
| " " | 10 h. | 0 m. | =13 |
| " " | | 1 | = 2 |

Now the shock happened at 9 h. 50 m. 6 s. at Charleston; hence 32 reports give a time distinctly too soon. But note that, though 31 gave the time at 54 minutes past 9 and 21 gave 56 minutes, as many as 68 gave 55 minutes. Similarly 13 gave 10 o'clock exactly, whereas only 3 gave 9 h. 59 m., and 2 gave 10 h. 1 m. These returns show a strong favouritism for 9 h. 50 m., 9 h. 55 m., and 10 h., the explanation of which is human, not seismic. By taking into account what Dutton regards as the really good time observations we are able to draw fairly well the coseismal of 3 minutes after the occurrence of the shock at Charleston. It is found to run close to the isoseism IV. The average distance of isoseism IV from Charleston is 585 miles; and

[1] *United States Geological Survey*, Ninth Annual Report, 1887–8.

this divided by 3 minutes, or 180 seconds, gives the transit speed 3·25 miles per second. Dutton, by applying the method of least squares to the two groups of Best Observations and Good Reports, obtained respectively $3{\cdot}236 \pm 0{\cdot}105$ and $3{\cdot}226 \pm 0{\cdot}147$. This is in itself a result of great value, but it helps in no way to find the depth of the earthquake focus.

Let $d$ be the depth of focus and $x$ the distance of a surface point $P$ from the epicentre $E$, as shown in Fig. 9 on page 40. Then, assuming the disturbance to be propagated with constant speed, $v$, through the earth's crust, we find that it will reach the position $x$ in time,

$$t = \sqrt{\frac{(x^2 + d^2)}{v^2}} \quad \text{or} \quad t^2 - \frac{x^2}{v^2} = \frac{d^2}{v^2}.$$

The graphical representation of this is a hyperbola with vertex at the epicentre, from which, when once drawn, we may determine both $v$ and $d$. But unfortunately neither this curve nor any modification of it obtained by assuming the speed of propagation to vary with the depth is at all applicable to practical cases. The depth of focus of any earthquake is probably comparatively small, never exceeding 20 or 30 miles. It is consequently only within a radius of 50 miles or so that the hyperbolic character of the time-graph could be detected. When $d$ is small compared with $x$, we have, by expansion of the square root in the first form of the equation,

$$t = \frac{x}{v}\left(1 + \frac{d^2}{2x^2} + \text{etc.}\right).$$

When $x = 4\,d$, the second term in the bracket becomes 1/32. With $d = 25$ and $x = 100$ miles, the ratio $x/v$ will be somewhere between 30 and 60 seconds. But very few time observations are correct to even 5 seconds, that is, to $\frac{1}{12}$th of the higher limit named. Consequently a difference of 1 in 32 is far within the errors of observation.

As a matter of fact the time-graphs of the Charleston earthquake of 1886, and of the Assam earthquake of 1897, are practically straight lines, giving the approximate value of $v$, but not giving the least suggestion of the value of $d$.

The time-graph of the Laibach earthquake of 1895, as drawn by F. E. Suess, shows a distinct concavity upwards with a point of inflexion at a distance of 250 kilometres. If, however, we take into account only those time observations which refer to the beginning of the shock, the time-graph is practically a straight line from distance 75 kms. to distance 350 kms. This is what we should expect for earthquake origins at depths not exceeding 40 or 50 kms.

In short, the determination of the depths of earthquake origins from time observations is at present a hopeless endeavour. Only those within the epicentral tract of a destructive shock could be of any use; but for the time records to be of any value they must be certain to 5 or 10 seconds. The average man, untrained in scientific research, is satisfied if he knows a time to within half a minute. Instrumental records alone will give satisfactory returns; but when the shock is severe the chances are that the instruments themselves will be wrecked. The evidence of stopped clocks has frequently been appealed to; but the careful sifting of this evidence by Dutton, in the case of the Charleston earthquake, did not lead to consistent results.

Dutton has himself suggested another method of getting an estimate of the depth of an earthquake focus. This depends on a consideration of the rate at which the intensity of the shock falls off as the disturbance spreads out in all directions from the focus and makes its effects apparent at the surface. The whole value of the method must ultimately depend upon the certainty with which we can ascertain the rate of decay of the intensity as the distance from the epicentre increases.

The arbitrary and essentially unscientific character of the determination of intensities in terms of individual sensations has already been referred to. It does not seem to be such a hopeless task to deduce dynamical conclusions from the overthrow and fracture of chimneys, monuments, tombs, walls, &c. It is important, then, to consider the dynamical conditions which determine these definite destructive effects. The problem was discussed by Mallet and his contemporaries; but little real advance was made till Milne began his

experiments in Japan in 1885. In that year he investigated the overturning of blocks and columns which were placed on a platform capable of being moved to and fro with simple harmonic motions of various amplitudes and periods. The results of the experiments are given in volume viii of the *Transactions of the Seismological Society of Japan*. In 1893 a more elaborate series of experiments was carried out by Milne, acting in conjunction with Omori;[1] and the latter has continued the investigation, making use of a platform which could be oscillated vertically as well as horizontally.[2]

In all these sets of experiments, columns of various dimensions and materials were set on the platform, which was then set in motion through various ranges and with various periods. The motion of the platform was at the same time recorded on a strip of paper moving steadily at right angles to the direction in which the platform was oscillating. From the sinuous record so obtained, the acceleration at which the overthrow occurred could be readily calculated, and used to test the applicability of various formulae for the overthrow of columns. The formula which stood best the test was the exceedingly simple one due to C. D. West, professor of mechanical engineering in Japan. Consider a block to be on the point of turning over by rotation round the edge A (Fig. 8). The effective force is the inertia of the block in relation to the horizontal acceleration of the ground. If $a$ represents this acceleration, the block relatively to the ground is acted on by a force $Ma$ through its centre of gravity in the direction opposite to that in which the acceleration of the ground is taking place, where $M$ is the mass of the block. Taking $x$ equal to the horizontal distance of the centre of gravity from the

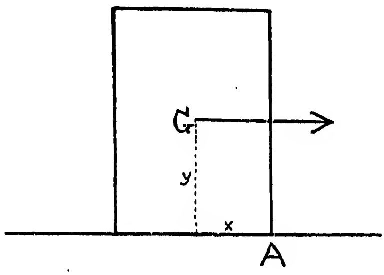



Fig. 8.

[1] See *Seismological Journal of Japan*, vol. i.
[2] See *Earthquake Investigation Committee Publications*, No. 4, 1900.

edge $A$ and $y$ equal to its height above $A$, we have, equating moments of forces,

$$May = Mgx \text{ or } a = gx/y$$

where $g$ is the acceleration due to gravity

In the latest experiments carried out by Omori, columns of iron, brick, and wood were used, their heights varying from a little under 1 ft. to fully 3 ft., and their sections square or circular, ranging from 4 ins. to 1 ft. in width. Some were hollow and some were solid: and of the forty columns experimented with, one only, a hollow column of wood, 10 ins. square and 20 ins. high, could not be overturned by the power available. The acceleration of motion varied from about 3/4ths to 11 times the acceleration due to gravity, that is from 24 to 350 ft. per second per second.

The general result of the experiments was that the ratio of the acceleration expressed by West's formula to the actual acceleration which overturned the columns varied from 1·5 to 0·7. In 29 cases the ratio fell between 0·8 and 1·2; and in 10 cases the ratio fell outside these limits.

We may therefore conclude that the overturning power of an earthquake shock may be approximately determined by means of West's formula, which applies to columns quite irrespective of their material.

Milne also initiated important experiments on the fracture of columns. The question was discussed by Mallet in his classic, *The Great Neapolitan Earthquake*, vol. i, page 141. By a slight modification of his formula we find, for the acceleration necessary to produce fracture in a column of thickness $2b$ ins. the expression

$$a = g\frac{IF}{bhW}$$

where $F$ is the tensile strength in pounds per sq. in., $I$ the moment of inertia of the area about the axis of fracture, $W$ the weight of the portion broken off, and $h$ the height in inches of the centre of gravity of the fractured part above the plane of fracture.

Both Milne's earlier results and Omori's later results show that the above formula is fairly applicable. Before the

expression can be used we must know the tensile strength of the material of the column which has been fractured by the shock.

Basing upon these results Omori has, from his observations of damage done in the Mino-Owari earthquake of 1891, constructed what he calls an absolute scale of intensities—a dynamical scale of intensities would perhaps be a preferable name. The idea is to express in terms of the amount and nature of the damage done the average accelerations of the earthquake motion. Holden has made a similar comparison, expressing the Rossi-Forel scale in terms of accelerations. Remembering that the comparisons are admittedly only approximate we may combine Holden's and Omori's conclusions in the following rough table :

| Rossi-Forel Scale | | Ratio of the Acceleration to that of Gravity | Amplitudes in mm. for periods of | |
|---|---|---|---|---|
| | | | 1 sec. | 2 sec. |
| I | slight | 0·02 | 0·5 | 2 |
| II | | 0·04 | 1·0 | 4 |
| III | | 0·06 | 1·5 | 6 |
| IV | weak | 0·08 | 2·0 | 8 |
| V | | 0·12 | 2·8 | 11 |
| VI | strong | 0·15 | 3·8 | 15 |
| VII | | 0·30 | 7·5 | 30 |
| VIII | | 1·00 | 25 | 100 |
| IX | violent | 2 | 50 | 200 |
| X | | 4 | 100 | 400 |

If in accordance with results already discussed (p. 30) we assume a linear relation between distance from epicentre and intensity as estimated on the Rossi-Forel scale, we shall find no clear evidence of a zone of comparatively rapid decay of intensity, such as is required for the application of Dutton's method. But in this respect each individual earthquake must be judged by itself. Thus Mr. Middlemiss, in his preliminary report of the Kangra (or Lahore) earthquake of April 4, 1905, finds evidence for a region of rapid decay of intensity in the neighbourhood of the isoseisms X and IX. The absence of isoseisms of higher name than X seems to me to make this determination very doubtful. There ought to be a distinct point of inflexion in the curve

of intensity such as Dutton indicates in his theoretical discussion of the method. This we shall now consider.

Assuming the disturbance to radiate out in straight lines from the focus and neglecting meanwhile loss of energy due to viscosity and fracturing, we have the energy at any surface point distant $x$ from the epicentre given by the formula

$$E = \frac{A}{(x^2 + d^2)}$$

where $A$ is a constant depending on the original energy of the shock. Measuring distance horizontally and corresponding energy vertically, we obtain a representative graph

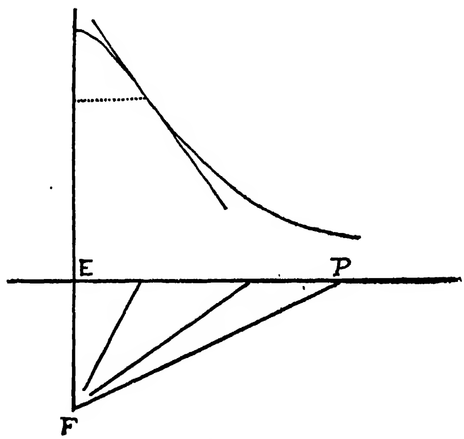



FIG. 9.

which begins convex upwards in the epicentral region, but after a certain distance becomes concave upwards. This curve is steepest at the point which separates the upward convexity from the upward concavity, and here accordingly the energy is decaying most rapidly per unit of distance outward from the epicentre. Dutton's energy expression being assumed, it follows mathematically that the point of inflexion which separates the convexity from the concavity and determines the position of the steepest energy gradient is at a distance $x$, which is connected with the depth of focus $d$ by the formula $d = x\sqrt{3}$.

But we estimate intensity by damage done to chimneys,

walls, monuments, &c.—damage which depends mainly upon the acceleration, or more accurately upon the horizontal component of the acceleration. In simple harmonic motion the acceleration is proportional to the displacement, while the energy is proportional to the square of the displacement. Hence it should be more correct or at least as reasonable to use, instead of Dutton's formula, the expression

$$\frac{A}{\sqrt{x^2+d^2}} \times \frac{x}{\sqrt{x^2+d^2}} = \frac{Ax}{x^2+d^2}.$$

But further because of viscosity and fracture there must be a more rapid fall off of displacement and acceleration than is given by this theoretical formula. A better approximation to the truth might be given by assuming a higher power of the quantity $(x^2+d^2)$. Let us put

$$I = \frac{Ax}{(x^2+d^2)^n}$$

where $n$ is any number, and find the relation between $x$ and $d$ which corresponds to the point of steepest gradient. This is found mathematically by equating to zero the second differential coefficient of the expression with regard to the variable $x$. This gives

$$d\sqrt{3} = x\sqrt{2n-1}.$$

When $n=1$ we have the simplest case in which all loss due to viscosity and fracturing is neglected, and we obtain $d\sqrt{3} = x$, instead of Dutton's value, $d = x\sqrt{3}$. For higher values of $n$ the ratio of $d$ to $x$ increases. The only result we can gain from this argument seems to be that the depth of the focus would be about equal to the average radius of the ring-shaped zone across which the intensity decays most quickly, provided no marked loss of energy occurred. But any probable assumption as to loss of energy due to viscosity and fracture will increase the ratio of $d$ to $x$. Hence we obtain simply a limiting value for the smallest possible depth of the focus. Applying Dutton's formula to the case of the Kangra earthquake, Mr. Middlemiss concludes that the long 50 mile focus lay at an average depth of from 18 to 30 miles, with a dip of 13° or 14° to the horizontal.

Quite apart from the numerical details which cannot but be vague and uncertain, the general principles underlying Dutton's discussion are of interest and importance. The deeper the focus the more extensive will be the epicentral tract bounded by the highest isoseism. A very shallow focus may produce great damage in a small epicentral area, but its intensity will decay rapidly with increasing distance. Such destructive but comparatively local earthquakes are frequently felt in certain parts of Italy. On the other hand, great earthquakes with deep-seated sources are felt over a wide expanse of country, and by delicate instruments can be traced all over the earth's surface. We shall return to this question of world-shaking earthquakes in chapters xi and xii.

In addition to the isoseismal and coseismal lines there is a third group devised by Dr. Davison and used by him with great ingenuity in interpreting certain phenomena of moderate earthquakes—namely, the ISACOUSTIC LINES. These are determined by the percentage of observers who heard as well as felt the shock. Thus the line marked 80 passes through all points in the vicinity of which 80 per cent. of the observers of the shock heard the earthquake sound. It is only in districts where there are many towns that this method can be utilized; for with few observers in a scattered community percentages are meaningless and valueless.

One of the most interesting earthquakes discussed by Davison is the Hereford shock of 1896. This was felt over practically the whole of England, the intensity of the shock in Hereford being sufficient to damage buildings and bring down chimneys. The highest isoseismal was No. VIII on the Rossi-Forel scale, and enclosed an oval-shaped area of 724 square miles, 40 miles long and 23 miles broad, the longer axis lying nearly NW. and SE. The chart constructed by Davison is here reproduced. The full lines are the isoseismal, and the dotted the isacoustic lines.

One well marked feature of the earthquake was the double shock, which was experienced in all places except those lying on a narrow curved zone passing from Devonshire

through Hereford towards the east. Its position is indicated by the non-closed dotted line on the chart. The simplest explanation is that given by Davison, namely, that there were two distinct foci originating disturbances almost, but not quite, simultaneously. The narrow zone of single shock is the locus of points at which the disturbances from the two foci arrived at the same instant. Theoretically, if we consider the earth's surface to be plane, this line should be a hyperbola, becoming a straight line only when the two shocks of the twin-earthquake (as it is called) are absolutely simul-

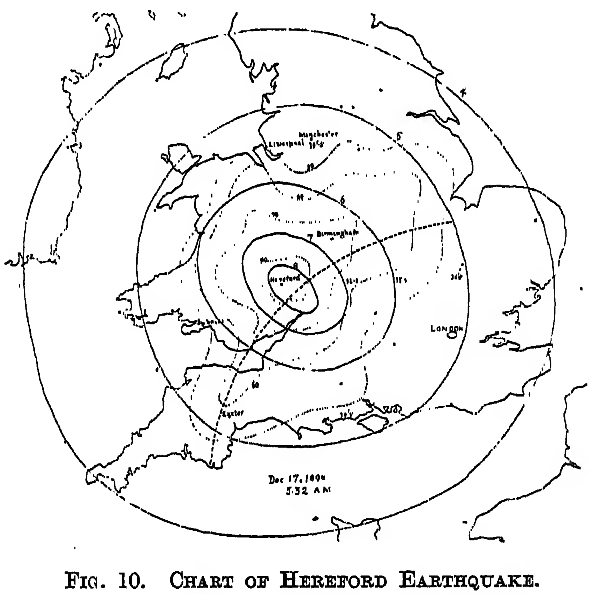



FIG. 10. CHART OF HEREFORD EARTHQUAKE.

taneous. This, for example, was the case in the Derby earthquake of 1903.

In the case of the Hereford earthquake the disturbance from the more northern focus was the stronger; and all the relative magnitudes and durations of the shocks and the interval between them, as experienced at different places, were well co-ordinated by means of this hypothesis of the two independent foci.

The whole subject of twin-earthquakes is discussed by

Davison in a paper published in the *Quarterly Journal of the Geological Society* for February, 1905. I quote his opening paragraph.

'The essential characteristic of a twin-earthquake is the existence of two maxima of intensity connected by weaker tremulous motion or the division of the shock into two parts separated by a brief interval of rest and quiet. This feature, however, is not entirely peculiar to twin-earthquakes; for, occasionally, one earthquake is succeeded by another so rapidly as to simulate a twin-earthquake in this respect. A closer investigation of the phenomenon shows . . . that the two parts or maxima of a twin-earthquake originate in two detached or practically detached foci; whereas in a double earthquake, the foci are either coincident or overlapping. . . . In all parts of the disturbed area the member of a double earthquake which occurs first is felt first. In a twin-earthquake, on the other hand, the second impulse may, but does not necessarily occur before the vibrations from the first focus have reached the other; so that over most of the disturbed area the vibrations first felt are those which come from the nearer focus, whether that focus was first in action or not. In a double earthquake the second shock is a consequence of the first; in a twin-earthquake each is independent of the other.'

Some of the arguments advanced by Davison may be open to criticism; but the general discussion of the broad features of twin-earthquakes is sound and convincing. The presence of the zone of simultaneous shock—the synkinetic band as he calls it—which separates two regions at every point of which two shocks were felt, is a feature of remarkable interest, inexplicable except on the assumption of two foci originating disturbances practically simultaneously. This synkinetic band will be convex towards the direction of the focus first in action. In virtue of the overlapping of the isoseisms due to the foci individually, the resultant isoseismal lines, as deduced from observation, present certain characteristics, the most noteworthy of which is the way in which the higher isoseisms lie excentrically within the lower ones. This feature is beautifully illustrated by Middlemiss's delineation of the Kangra earthquake already

referred to. A small portion of this is reproduced, showing apparently a double system of twin-earthquakes.


Fig. 11.

The isoseism VIII consists of two oval-shaped lines 20 to 50 miles apart ; and within the larger of these the higher isoseisms IX and X are situated excentrically.

Davison has suggested a theory of the origin of twin-earthquakes, connecting their occurrence with the formation of the foldings of the crust, and the average distance apart of the two foci with the average distance between successive anticlinal and synclinal folds.

The present chapter would not be complete without some reference to E. Harboe's ingenious attempt to bring harmony out of the chaos of time records of great earthquakes.[1] For this purpose Harboe imagines the existence of what he calls 'Herdlinien' or focal lines. These may be regarded as the surface projections of the more or less deep-seated lines along which the shocks which constitute a given earthquake have their origin. These lines are to be laid down from a careful consideration of the noted registered or recorded times at which the earthquake shock was first felt at the various localities of the earthquake-visited district, special attention being paid to the episeismic tract, where, with the greater intensity of shock, the times will no doubt be more accurately determined. The trend of the isoseisms, when these are delineated with sufficient fullness and accuracy, is also to be taken into account. Harboe applies his method in particular to the Charleston earthquake, the Assam earthquake, the Kumamoto earthquake of 1889 and one or two others of smaller intensity. The ideas which are at the foundation of the method are unquestionably of great value and seismologically sound. Unfortunately the working out of any case in detail is hampered by many grave difficulties. First and foremost, there is the lack of accuracy in time measurements, referred to above, especially in connexion with the Charleston earthquake itself. As regards the particular development of focal lines indicated by Harboe in his discussion of this earthquake I am quite unable to understand how he obtains the focal lines which run out into the sea. But again, even suppose we had absolutely accurate time observations of the first noticeable tremor which heralds in a shock—an ideal which is attainable only with instrumental records—how are we to take into account those local conditions referred to by Harboe himself. As Kusakabe

[1] See Gerland's *Beiträge zur Geophysik*, Band V, pp. 206–38 (1903).

has pointed out, the differences in elasticity and viscosity of contiguous strata in a shaken district will bring in discrepancies in time measurements and dynamical estimates of intensity which may quite mislead when the attempt is made to lay down Harboe's 'Herdlinien'. Apart altogether from the examples given of his method, Harboe's ideas are, however, important as a timely argument against the too prevalent conception of an earthquake having a limited 'centre' of disturbance.

# CHAPTER IV

## INSTRUMENTAL SEISMOLOGY

Instrumental Seismology. Development of Seismographs in Japan and Italy. Realization of the Steady Point. Stevenson's Aseismic Joint. Vertical Pendulum. Forbes's Inverted Pendulum. Ewing's Duplex Pendulum. Horizontal Pendulum. Bracket Seismograph. Vertical Seismograph. Record of Complete Motion. Milne's Horizontal Pendulum. Omori's Horizontal Pendulum. Wiechert's Seismographs. Tanakadate's Vertical Seismograph. Trustworthiness of Record.

In the preceding chapters we have referred to the complicated motion of the ground shaken by an earthquake. This was proved by the different rotations experienced by neighbouring pillars, as well as by the difficulty the seismologist experienced when he tried to co-ordinate all the observed changes and effects with a view to determine the origin and intensity of the shock. The scientific mind, however, is not content with those vague indications, but sets itself to invent a means of absolutely measuring the motions as they occur during the earthquake. Instruments intended for this purpose are called Seismometers; and the best seismometers are also seismographs, since they give a permanent record of the motion.

The earliest types of seismometer were hardly worthy of the name, being simply seismoscopes, or instruments for recording the occurrence of an earthquake. The seismometer, properly so-called, came into being about thirty years ago, being rapidly evolved at the hands of the band of enthusiasts whom Europe and America sent to Japan in the early days of her awakening. Particularly effective in this direction were the labours of Ewing, Gray, and Milne, who may be said to have created the seismograph as a scientific instrument of research.

Meanwhile in Italy the same subject was receiving attention at the hands of Agamennone, Cancani, Vicentini, and Grablowitz.

The Japanese took up with characteristic energy the work

initiated by their early teachers and advisers, established a chair of seismology in their university, and encouraged in every way the theoretical and practical study of earthquakes.

It is not my intention to give a detailed account of any one of the instruments now in use in seismological observatories.[1] It will suffice to indicate the fundamental principles of construction and to give a general account of the more successful devices for obtaining a continuous record of a shock. Historic references will be made only incidentally.

The mechanical problem to be solved so as to be able to measure the earthquake motion is to find a point which shall be motionless during the whole motion of the ground. How is this 'steady point' to be realized? It is obvious that in some way we must make use of the property of inertia, in virtue of which a body tends to remain in its state of rest. That state of rest ceases to exist as soon as the body is acted on by some force; and since the body must have some connexion with the earth it is not possible absolutely to fulfil this condition of unchanging rest. It is not difficult, however, to arrange a mechanism so that a certain point of it will not immediately respond to the influence of the movements of the ground with which the mechanism is of necessity in contact at some part of it. The numerous forms of seismoscopes for indicating the occurrence of an earthquake are constructed on this simpler principle, and they are historically the forerunners of the continuously recording seismograph which gives a record indicating to a greater or less extent the whole motion of the ground during the passing of the shock.

A few of the more important methods may be briefly described.

There is, for example, the ball and plane combination, in which a plane horizontal surface rests on three spherical balls which roll on another plane horizontal surface. When

[1] A very complete account to date of the most important Seismographs and Seismometers is given, together with a full bibliography, by Dr. R. Ehlert in *Gerland's Beiträge zur Geophysik*, vol. iii, 1896–8.

the latter moves with the ground, the inertia of the supported body tends to keep it in position, the balls rolling on the lower plate in the direction contrary to that in which the said plate moves. Verbeck, in Japan, adopted this method; and D. and T. Stevenson, the well-known lighthouse engineers, made the method the foundation principle of their 'aseismic joint'. When in 1868 this firm was invited by the Japanese to put up lighthouse towers round the Japanese coast, they were at the same time asked to guard against the effects of the numerous earthquakes which occurred in that country. To this end they proposed to support a conical tower sixty feet high and forty feet in diameter at the base upon six steel spheres 4·5 inches in diameter. Each sphere rested in a shallow concavity, and a similar concavity on the lower surface of the base of the tower rested on the sphere. The lighting apparatus was independently supported on similar balls. The system proved serviceable in places particularly subject to earthquakes; but at most lighthouses the shocks were found not to be so frequent or so serious as to require the use of the aseismic joint.

The same principle was utilized by Gray in his rolling sphere seismograph. The base was part of a spherical surface, and near the centre of the sphere of which the surface was a part a heavy weight was pivoted in such a way that the pivot point remained practically steady when the earth's surface moved to and fro under the spherical base, making it to roll. A lever passed from the pivot through a point on a bracket fixed to the earth and then downwards till it met a smoked glass surface over which it moved, giving a magnified record of the relative motion.

Another important means of getting a temporarily steady point is to use a pendulum with a massive bob. This massive bob tends to remain steady when the ground is in motion. To get satisfactory results from the pendulum seismograph it is necessary to use a long pendulum, and the longer the better. The Italian seismologists have made the pendulum the basis of their elaborate instruments. A heavy mass, weighing it may be 200 pounds or more, is

suspended by three metallic suspension rods, which in certain cases are about 50 feet long. These rods are united above to a brass cap which hangs by a steel wire. Such a pendulum will have a to-and-fro oscillation of about eight seconds period. The point from which the pendulum hangs is of course a part of the earth and will partake of the motion of the ground. Under certain conditions, frequently realized, this motion begins to set up a proper motion in the pendulum bob, which can then be no longer regarded as a steady point. There may be a marked resonance effect (see below, pp. 72, 73). The quick vibratory motions which characterize many earthquakes will produce very little resonance, and for these the pendulum may be regarded as a fair approximation to a steady point.

The relative motion of the ground and the pendulum bob is recorded by means of a light lever set vertically immediately below the bob. The upper end of this lever is fixed to the bob and is the fulcrum. A point a little below is in connexion with a support which is fixed to the wall of the observatory, and this point accordingly moves with the earth. The lower end of the lever is prolonged so as to multiply the relative motion about five times. This motion, which experience shows to be very complex, takes place in all azimuths; but by an ingenious device it is decomposed into two rectangular components by means of two other levers which are influenced by the first. The further ends of these two levers trace out their movements on steadily moving strips of blackened paper; and in this way a record of the relative horizontal motion of pendulum bob and ground is obtained magnified about fifty-fold.

To get the vertical motion Vicentini adopts a method which will be described later when we come to discuss the instruments invented by the British investigators in Japan.

Agamennone's vertical pendulum seismograph is identical in principle with Vicentini's but differs greatly in detail. The recording levers are set above the heavy cylindrical bob, and are arranged to make two sets of records, one on

slowly moving paper and another on rapidly moving paper. For the sake of economy the rapidly moving paper is started automatically by the earthquake vibrations, which through their influence on a delicate seismoscope makes an electric connexion and starts the driving clockwork.

As already stated, the problem is to get a steady point, with respect to which the relative motion of the ground may be recorded and measured. As a practical piece of mechanical construction this resolves itself into the problem of adjusting a body in a position of equilibrium of small stability. The position must be one towards which the body after displacement tends to return; but the restoring force must be as small as possible, and the natural period of swing as long as possible, consistent with the other conditions which must be practically realized. For example, in the case of the vertical pendulum the stability is diminished by making the pendulum very long. But since the period varies with the square root of the length of the pendulum, it is obvious that there are practical limitations to an increase of the period by this means. Thus to get a pendulum to make its half swing in ten seconds we should have to make the length 100 times the length of the seconds-pendulum, that is nearly 4000 inches or 330 feet, a length altogether out of the question. This, in fact, is the one disadvantage of the vertical pendulum seismograph, that we must be content with a stability involving a period small enough to be frequently met with in earthquake vibrations.

There are, however, other methods of attaining a long swing period, of which the Duplex Pendulum and Horizontal Pendulum Seismographs are the embodiment.

In 1841, in a paper communicated to the Royal Society of Edinburgh, Professor J. D. Forbes, after commenting on the inadequacy of the ordinary pendulum for indicating earthquake movements, describes a form of inverted pendulum seismometer, which is of interest historically as the first scientific attempt to get an approximately steady point. One of the instruments constructed by him is now in the Natural Philosophy Museum of Edinburgh University.

It was intended to be used at Comrie in Perthshire for registering the first shock of an earthquake.

His description, slightly paraphrased, is as follows :—A vertical metal rod, having a ball of lead movable upon it, is supported upon a vertical cylindrical steel wire, which is capable of being made more or less stiff by pinching it at a shorter or greater length by means of a screw. By adjusting the stiffness of the wire or the height of the ball we may alter to any extent the relation of the forces of elasticity and gravity, and consequently render the equilibrium in a vertical position stable, neutral, or unstable. The vibratory motion of the pendulum relatively to the ground is registered by a pencil placed in the prolongation of the metal rod and adjusted so as to trace the relative motion on a spherical dome of copper lined with paper.

Forbes works out the mathematical theory of the instrument in considerable detail. Two forms of the instrument were constructed, a large and a small, and were set up at Comrie; but apparently no results of value were got from them, probably because the Comrie earthquakes were neither numerous nor strong enough. Nevertheless Forbes clearly recognized the dynamical principle by means of which the sensitiveness of the pendulum could be almost indefinitely increased.

For further development along these lines we must go to Tokyo in the early eighties. Various suggestions were made by Gray and Ewing; but the most successful method for reducing the stability of a short pendulum was that devised by Ewing and known as the Duplex Pendulum.

'It consists of a combination of a common with an inverted pendulum. The common pendulum is stable: the inverted, with a rigid pivoted supporting rod, is unstable: by placing an inverted pendulum below a common pendulum and connecting the bobs so that any horizontal displacement must be common to both, we may make the equilibrium of the jointed system neutral or as feebly stable as may be desired.' Several forms of this instrument were devised by Ewing, and other modifications and improvements were effected by Milne. The aim of these modifications

was to make the instrument more compact, simplify the method of recording the movement, and diminish as far as possible the friction at the points and surfaces.

A diagram of Milne's latest form is reproduced from his original description in the *Transactions of the Seismological Society of Japan* (vol. xii, 1887).

The vertical pendulum consists of a heavy ring *W* sus-

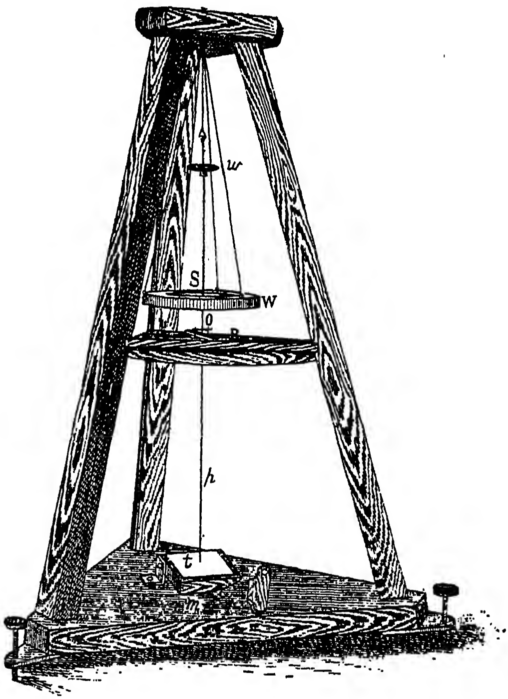



Fig. 12

pended by three threads. The inverted pendulum is pivoted at *O* a little below *W* and passes through a cross bar in it, being kinematically connected with the ring by means of a ball and socket joint. It is continued above and is adjusted by means of a small brass weight which can be screwed to the rod in any position. By a suitable arrangement the rod of the inverted pendulum is continued downwards past the pivot and forms a multiplying

lever the lower end of which rests lightly on a surface of smoked glass $t$. When the tripod stand is shaken the pivot $O$ moves relatively to the bob $W$, and this motion is magnified in the tracing produced on the smoked glass.

By setting a duplex pendulum on a rocking table, the motion of which was registered relatively to the ground at the same time by a lever constructed on the same principle as the prolongation downwards of the inverted pendulum just described, Milne made a series of interesting experiments which tested the accuracy of the record given by the duplex pendulum. The comparison was very satisfactory, showing that the duplex pendulum record reproduces with fair accuracy any to-and-fro horizontal motion of the ground. When we come to discuss Galitzin's more recent experiments of the same nature, we shall find that the reason for this fairly faithful reproduction of the motion of the ground is due to the relatively high amount of friction involved in the writing of the record.

The records of the duplex pendulum are very confused, and little can be made of them except the direction or directions of greatest movement and the amount of the movement. It is not possible to determine the periods involved. The instrument has more the character of a scientific toy than of a really efficient seismometer.

As a means for obtaining a complete record of the whole horizontal movement the duplex pendulum is far inferior to the bracket seismograph or horizontal pendulum, the successful application of which to earthquake measurement was first accomplished in Japan by Ewing.

The principle of the horizontal pendulum when the movements are horizontal is a simple piece of dynamics. Imagine an impulse $I$ to act on a mass $m$ at a distance $x$ from the centre of mass. Its effect is to start the centre of mass $G$ with a velocity $I/m$ in the direction of the impulse, and to set up an angular velocity equal to $Ix/(mk^2)$ in the plane containing the centre of mass and the impulse. Here $k$ is the radius of gyration and $mk^2$ the moment of inertia with reference to the axis through $G$ perpendicular to the plane just referred to.

Now this combination of motions of a rigid body can be represented by a rotation about a definite axis known as the instantaneous axis of rotation. It will pass through a certain point $C$, such that the distance $CG$ multiplied by the angular speed will be equal to the linear speed of $G$. Hence

$$CG = \frac{\text{linear speed}}{\text{angular speed}} = \frac{I}{m} \div \frac{Ix}{mk^2} = \frac{k^2}{x}.$$

This point or line $C$ is therefore for the moment at rest, and if we connect it in some manner with the ground we shall be able to observe and record the relative motion of the two, which *at the start* is the real motion of the ground.

The principle was practically realized by pivoting a heavy body so as to be capable of motion about a nearly vertical axis. The ends of this axis were pivoted to supports in connexion with the ground; and from the heavy body a light lever was carried so as to give a magnified record of the relative motion of the body and the earth. It was necessary to place the axis a little off the vertical, leaning in fact somewhat towards the heavy body, so as to give to the heavy body and recording lever some amount of stability. Otherwise it would have been impossible for the horizontal pendulum to retain a definite azimuth. Unfortunately, as we shall see below, the existence of a certain degree of stability brings in the phenomenon of dynamical resonance.

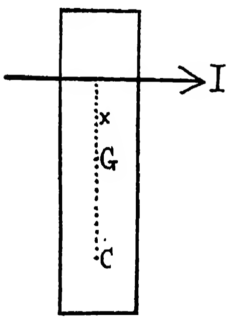



FIG. 13.

One instrument of this kind is sensitive to motion perpendicular to the line joining the axis of suspension and the steady point. To obtain a complete measurement of the horizontal motion of the earth we must have two such instruments set in perpendicular directions. The horizontal pendulum which points north or south will show the east-west movement, and the one pointing east or west will show the north-south movement.

In Ewing's form of instruments the ends of the recording levers trace out concentric circles on a revolving glass plate

kept in motion for a particular period of time by means of clockwork. The first tremors of an earthquake acting on a delicate seismoscope complete an electric circuit, which releases the glass plate and allows the clockwork to drive it for four or five minutes. The pointers, resting lightly on the smoked surface, trace out a sinuous line oscillating about the continuous circle which would have been traced had no earthquake been in action. The one inconvenience of this form of recorder is that with a quake lasting a longer time than the period of revolution of the glass plate the tracing by any one pointer will begin to over-write itself, and there may be some difficulty in disentangling the separate parts of the mingled record. It was with such an instrument, however, that the first satisfactory records of earthquake motion were obtained.

With two horizontal pendulums or bracket seismographs recording simultaneously on the revolving plate, the components of the horizontal motion only were recorded. It was essential to record also the vertical motion. This proved a more difficult problem than the recording of the horizontal movement. Indications of vertical motion were first obtained by the use of vertical springs variously loaded; but such spring-suspended loads once started were set oscillating up and down with their own periods of oscillation, and were thus useless for giving any real continuous record of an earthquake motion. Suppose, for example, that the load hangs at the end of a vertical spring. To make a load suspended in this way at all efficient as a seismoscope it is necessary to increase its period of vertical oscillations to the utmost. But the period of a heavy load at the end of a vertical spiral spring depends, *inter alia,* upon the amount by which the spring is extended by the load. To get a slow period we must use a very long spring, so that the amount of extension is large. Clearly this method is practically incapable of useful development.

We owe to Thomas Gray[1] an ingenious modification of the spring suspension by which the problem of getting a steady point for vertical motions was solved. Instead

[1] *Transactions of the Seismological Society of Japan,* vol. iii, 1881.

of hanging the heavy body to the end of the spring, Gray used the spring to keep in a horizontal position a weighted rod with one end pivoted to a vertical support. Let $O$ (Fig. 14) be the pivot, $A$ the point of fixture of the spring, and $G$ the centre of gravity of the weighted rod. For any small motion about the horizontal position it is easy to see that the moment of the weight about $O$ remains practically constant, but that the moment of the force of the spring diminishes or increases, because of the change of length, according as the motion is up or down. It is therefore not possible to get a sufficiently small stability by this means. By suitably fixing to the rod a trough containing liquid, the to-and-fro motion of which altered the distribution of weight, Gray was able to get as small a stability as was required. A consideration of the fundamental principle involved in this arrangement led Ewing a few months later to a simpler solution of the same problem. What was wanted was to arrange matters so that the upward elastic pull of the spring and the downward pull of gravity should have equal moments about the pivot for all small motions in the neighbourhood of the horizontal position of the rod. It is curious to note, as an example of the circuitous path by which the human mind frequently attains its object, that Ewing's solution of the problem would have come at once if it had not been for the apparent simplicity of attaching the spring to a point in the line joining the pivot and the centre of gravity. In short, the less simple arrangement, in which $O$, $A$, and $G$ are not in the same line, proved the simpler in the end for the purpose aimed at.

The principle may be demonstrated in the following simple manner. Let $OAG$ represent the rod and weight with centre of gravity at $G$, pivot at $O$; and let the spiral spring be attached at $A$, which does not lie in the line $OG$. Then it is clear that an upward motion into the position $A'OG'$ indicated by the dotted lines will, while shortening the spring, increase the leverage about $O$. In like manner for a downward motion the lengthening spring will act about $O$ with a smaller leverage. By a proper adjustment the changing value of the force of the spring due to change

of length will be accompanied by a change of distance from the pivot, so that for small displacements the moment of the elastic pull will be as constant as the moment of the weight.

With the invention of the vertical motion seismometer it was possible now to obtain a complete record of an earthquake movement. This was first accomplished by Ewing, who arranged two bracket seismographs and the vertical motion seismometer to record by means of light levers on the same rotating smoked glass plate. Each pointer traced out a sinuous record by removing the lampblack from the part acted on. The record was then transferred to a sheet of sensitive paper by simply laying the glass plate over it in daylight, and developing and fixing in the usual way.

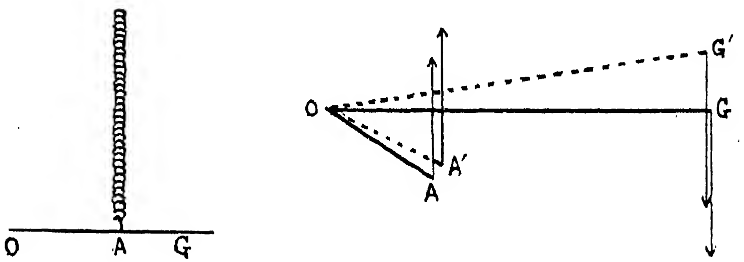



FIG. 14.

One of these complete records is shown in Fig. 15, very much reduced in size in the ratio of 4 : 21.

As already noted, the overlapping of successive parts of the record of a prolonged shaking is a disadvantage in the original form of revolving glass plate adopted by Ewing. In some forms of instruments the record is taken on smoked paper wound round a revolving cylinder, to which is given a slow translational motion in the direction of its axis. Probably the best method is to draw a strip of paper over the drum, so that the whole record is obtained in a single continuation. Such mechanical methods of taking the record introduce of necessity a certain amount of friction; and inventors have as a rule tried to reduce the frictional effects to a minimum.

The ordinary Ewing or Milne-Gray seismographs have proved very serviceable in Japan for recording the fre-

quently occurring moderate earthquakes; but it was soon found necessary to get still more delicate instruments if the smaller motions and pulsations of the ground were to be recorded. In the development of these more sensitive

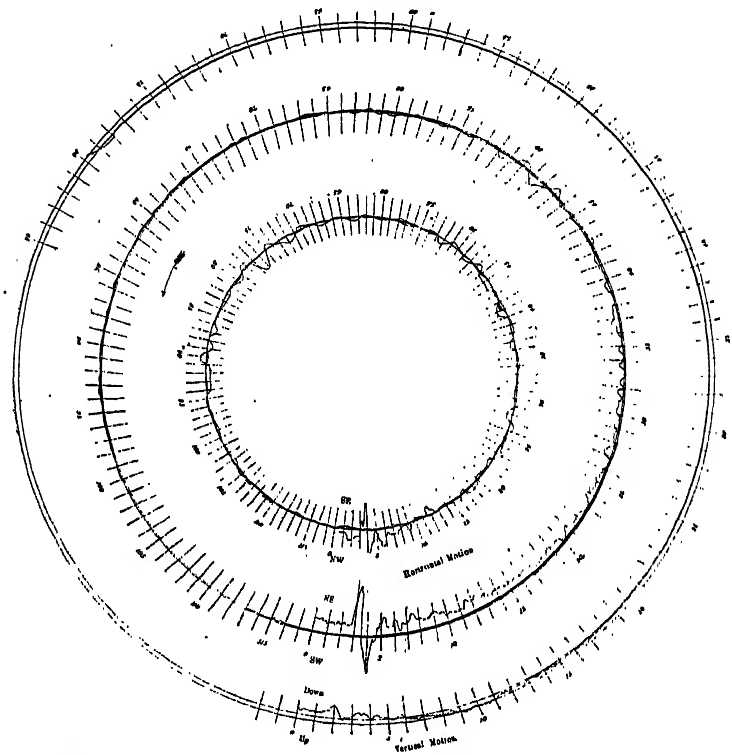



Diagram of Horizontal and Vertical Motion of the Semi-destructive Earthquake of June 20, 1894 (Tokyo). The wave lines on two inner circles indicate the Horizontal, and that on the outermost circle the Vertical Motion, all in actual dimensions. The plate revolved once in 118 seconds: the numerals on short radial lines mark the successive seconds of time from the beginning.

FIG. 15. COMPLETE EARTHQUAKE RECORD (EWING INSTRUMENT) REDUCED 4:21.

forms of seismograph the method of support of the horizontal pendulum has been modified in a way first suggested by Gray. Instead of a rigid frame for the support of the heavy body, the horizontal pendulum was pivoted at its one end and was retained in the horizontal position by means of a tie stretching from the neighbourhood of the

heavy weight to a point nearly vertically above the pivot. The nearer the point of attachment to a truly vertical position above the pivot the more delicate and the less stable will the horizontal pendulum be, and the better fitted for recording small motions. An important form of instrument is that devised by Milne and used for recording the far travelled tremors of large earthquakes. These will be discussed in chapters xi and xii; but here it is convenient to describe the instrument.

Milne's Horizontal Pendulum consists of a nearly horizontal boom suitably weighted and pivoted with as little friction as possible on its one end. The boom is supported by a tie which is fixed to a point nearly but not quite vertically above the pivot. By means of levelling screws on the base plate supporting the upright on which the boom pivots, the boom can be accurately directed towards a given direction and its small stability can be adjusted to a convenient amount. The weight attached to the comparatively light boom acts as a steady point; and any horizontal motion at right angles to the boom, as well as a tilting motion about an axis parallel to the boom, may be recorded if there is sufficient magnification of the relative motion of the boom and the upright on which it pivots.

In the Milne form of instrument, the boom is continued beyond the tie a considerable distance, and the outer end carries a small horizontal disk of blackened mica, which has a slit in it cut parallel to the boom. This mica will move relatively to the ground with a motion which is a magnification of the relative motion of boom and ground. It is arranged to swing over a slit in the lid of a box, this slit being at right angles to the slit in the mica. In the box a band of bromide paper is driven by clockwork so as to be passing continuously under the slit. The light from a small benzine lamp is reflected by means of a mirror downwards through the two slits and falls on the paper as a point of light. When the boom is steady this point of light traces what becomes after development a straight line on the moving paper. When the boom is in motion relatively to the fixed earth the line traced out on the

moving paper is of a more or less sinuous character, reproducing faithfully the to-and-fro motion of the boom relatively to the earth.

The time record is given by a watch with a long minute hand tipped with a piece of blackened mica, which once an hour eclipses the light entering the one end of the slit on the box. In some forms two slits are made on the mica disk, one broad and the other narrow. The fine line gives

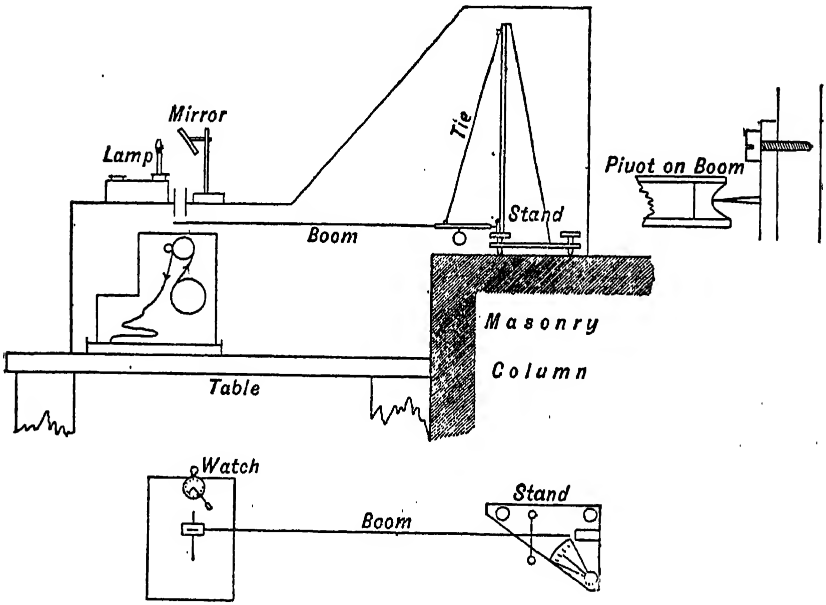



FIG. 16.

beautiful definition for slow or moderate movements, but may fail to produce a photographic impression when the motion is rapid. In this case the broader line is necessary.

The sensitiveness is adjusted until the period of free oscillation of the boom is about 15 seconds.

Some examples of records taken by the Milne Horizontal Pendulum are given below in chapters xi and xii.

The advantages of Milne's form of instrument are its comparative cheapness, its simplicity of construction, and the ease with which it can be installed and attended to. Its

sensitiveness is just about sufficient for the purpose aimed at, which is to obtain records of motions due to distant earthquakes. Its freedom from frictional restraints renders it quickly responsive to slight movements of the ground.

A more substantial form of horizontal pendulum has been constructed by Omori; and as he and his assistants have with its aid carried out investigations of the highest importance, it is necessary to give a description of its salient features. A diagram is shown in Fig. 17 one-tenth natural size.

In designing his instrument Omori aimed at constructing a seismometer capable not only of recording ordinary earthquakes of the moderate type frequent in Japan, but also of registering the smaller and slower movements of which the Ewing or Milne-Gray type of seismometer failed to give any record. For reasons which need not be given here, Omori decided to use the mechanical and not the photographic method of taking the record. This is effected by means of a light lever designed and constructed with great attention to mechanical details. It is pivoted about a vertical axis fixed to the ground in the neighbourhood of the heavy weight which forms the steady point of the horizontal pendulum. The pendulum itself is suspended in the same manner as in Milne's instrument, the mechanism for adjustment being planned with great care. The longer arm of the writing lever is suitably weighted with a small mass and presses lightly on the smoked paper wound round the revolving drum. The shorter arm of the writing lever is in contact with the heavy weight. Any relative motion of the horizontal pendulum and the ground is increased in the ratio of the arms, namely 1 to 10. A diagram of the essential connexion is given below on p. 70, where the theory of the instrument is under more particular consideration. The instrument is more massive than Milne's form, and requires for its support a well-built basement. The cylinder is run at a fairly high speed, so that beautiful open records are obtained. Some examples of these are given in Figs. 37 and 38.

Dr. E. Wiechert of Göttingen, who has in recent years given a great deal of attention to the theory and practical

details of seismographs of all kinds, has designed several new and improved types. In one of these a weight of 17,000 kilograms is suspended by three fairly thick iron

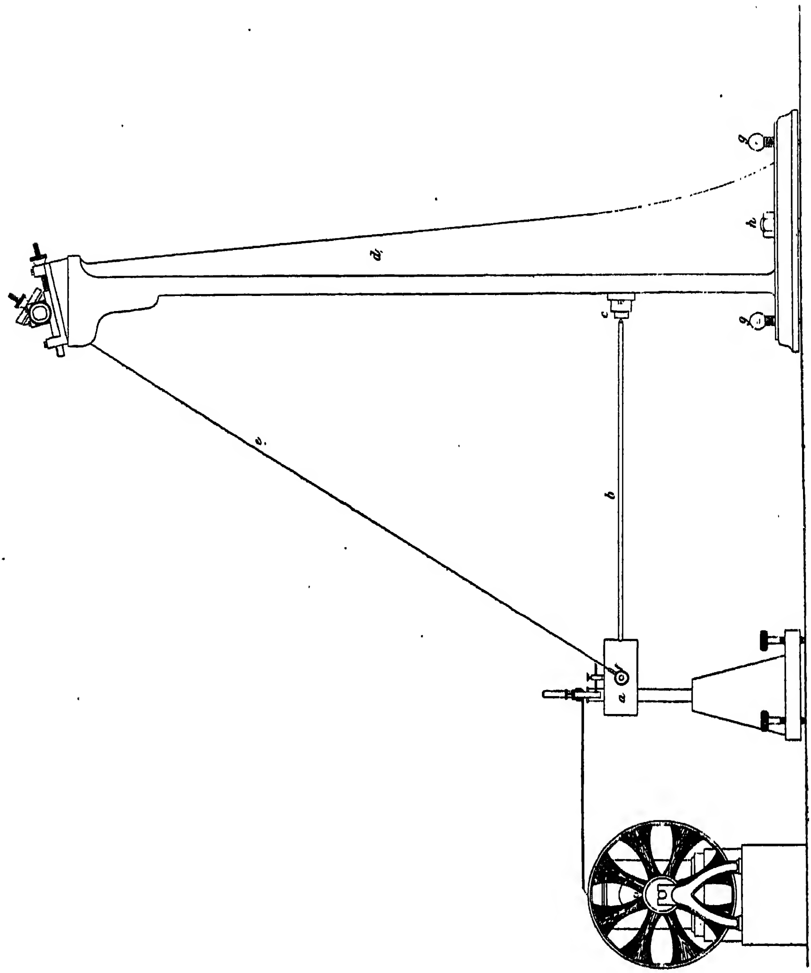


FIG. 17.

rods, the upper ends of which are fixed to an iron frame which moves with the earth. The heavy mass which acts as the steady point has sufficiently free horizontal movement because of the elastic yielding of the suspending rods.

The relative motion may be magnified several thousand times. The greatness of the mass which forms the steady point renders the effects of friction at the supports and at the mechanical registering apparatus practically insignificant. In his memoir on the theory of automatic seismographs, published in 1903,[1] Wiechert gives the dynamical theory of seismographs of all kinds of forms, and his instruments are the practical outcome of these important studies. Especially noteworthy is his introduction of a controllable method of 'damping' the oscillations, the necessity for which is discussed in the next chapter.

In an interesting modification of the vertical motion seismograph, due to Tanakadate, two spiral springs are used instead of the long vertical helical springs. These spiral springs act with opposite moments on the ends of the weighted horizontal bar which constitutes the steady point. The necessary sensibility is approximately obtained by winding up the spiral springs so as to act with the appropriate moments, and is finally adjusted by means of sliding pieces on vertical rods attached to the ends of the horizontal bar. The effects both of horizontal motion and tilting are perfectly eliminated.

In regard to all forms of seismograph the obvious criticism is, to what extent are their records a faithful reproduction of the motion of the ground? As stated above, Milne tested the action of the duplex pendulum by placing it on a platform which could be made to imitate the to-and-fro motion of an earthquake. Others have used the same tests; and in the case of most of the better forms of the less delicate seismographs for recording the movements accompanying felt earthquakes the seismograph has responded with fair accuracy to the motion of the ground. Recently Prince Galitzin has made an interesting series of tests of the same nature in connexion with his elaborate discussion of the dynamical theory of the horizontal pendulum. His conclusions deserve the fullest consideration.

[1] *Abhandlungen der Königl. Gesellsch. der Wissenschaften zu Göttingen.*

# CHAPTER V

## SEISMOMETRY

Dynamical Theory of the Horizontal Pendulum. Analysis of Motions. Tilting and Horizontal Movement inseparable. Galitzin's Discussion. Effect of Frictional Resistances. Omori's writing Lever. Galitzin's Experiments. Galitzin's Results. Theory of Forced Vibrations. Distinction between Static and Kinetic Sensitiveness. General Conclusions. Application to Seismograms. Untrustworthiness of Horizontal Pendulum Records.

THE dynamical theory of the horizontal pendulum is not simple, although the main features of its action are readily enough understood. Approximate solutions have been given by various investigators. We owe the most complete discussions to Wiechert and to Galitzin, who took up the subject almost simultaneously and quite independently of each other. Wiechert's memoir has been referred to in the last chapter. Galitzin's two memoirs, in addition to general discussions having much in common with Wiechert's investigation, give in a masterly manner not only the dynamical theory of the horizontal pendulum but also the experimental test of the theory. The first *On Seismometrical Observations* appeared in 1902, and the second *On the Method of Seismometrical Observations* in 1904.[1]

To fix our ideas let us suppose that the horizontal pendulum is set so as to point north and south, and that the line $PT$ from the pivot $P$ to the point $T$ of attachment of the tie makes a small angle of inclination $i$ with the vertical. If the effective length $HP$ of the horizontal pendulum is $l$, the period of free oscillation is given by the formula

$$T = 2\pi\sqrt{\frac{l}{gi}}\cdot$$

When the boom $HP$ is slightly displaced it will move to and fro in this period. It is clear that the period may be

[1] *Comptes Rendus des Séances de la Commission Sismique Permanente* (Imperial Academy of Sciences, St. Petersburg), vol. i.

greatly lengthened by making $i$ small enough. It is this power of adjustment of stability which gives to the horizontal pendulum its chief merit as a seismometer. I propose to call the angle $i$ the stability angle.

The kinds of movement of the ground to which this instrument will obviously respond may be classified thus :

(1) a rotation about the vertical axis,

(2) a horizontal movement of the ground in an east and west direction,

(3) a tilting of the ground about a north and south axis.

Each of these will produce an apparent displacement of the boom of the horizontal pendulum. But if these movements exist, the sensitiveness of the instrument and therefore the nature of its indications will be influenced by movement of the ground in a north and south direction and by a tilting about an east and west axis. For example, this tilting will alter the angle of inclination $i$ and will therefore change the stability of the pendulum.

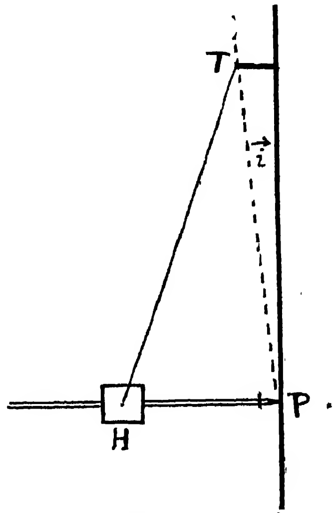


FIG. 18.

For a complex movement of the ground the behaviour of the horizontal pendulum will depend more or less on all these parts of the motion which have been named, namely, the components of the horizontal movements, and the rotations about the three axes, north-south, east-west, and vertical. Of these the three movements tabulated above are of the nature of forced vibrations impressed upon the horizontal pendulum. We shall distinguish them as the primary disturbances, the other movements which influence the behaviour of the horizontal pendulum being of a distinctly secondary significance.

When, as in the case of the bracket pendulums used in Japan for recording moderate earthquakes, the inclination $i$ is not very small, so that the period is about 3 or 4 seconds,

the secondary effects are insignificant. But when the sensitiveness of the instrument is increased until the period of free oscillation exceeds 12 or 15 seconds, these secondary effects cannot be neglected; and when, as in Omori's form of horizontal pendulum, the period is as great as one minute, the secondary effects due to tilting about the horizontal axis at right angles to the boom, or to movements of the ground in the direction of the boom, may become very significant.

As regards the primary effects, Galitzin strongly emphasizes the fact that it is impossible to separate the effect of the purely horizontal movement from that of the tilting. It is not possible, in fact, to draw any sure conclusions as to the motion of the ground from the indications of the records obtained with the horizontal pendulum as ordinarily constructed. If purely horizontal motion or a purely tilting movement alone existed some approximate conclusions might be deduced; but when both exist simultaneously their effects cannot be separated.

The mathematical proof of this depends on the fact that, in the equation of motion of the horizontal pendulum pointing north, the term which contains the acceleration of the east-west motion also contains a multiple of the angular displacement about the north-south axis. If we represent the acceleration by the letter $a$ and the angular displacement by the angle $\phi$, the term is of the form $a+g\phi$, where $g$ is the acceleration due to gravity. Suppose for simplicity that each is of harmonic type and, as is highly probable, of different period. Then each will be a forced vibration producing on the horizontal pendulum an effect which will depend on the value of each period as compared with the free vibration period of the pendulum. The effects produced on the pendulum by these coexisting forced vibrations will therefore not be simply proportional to the amplitudes of the corresponding earth movements.

A point which is considered carefully by Galitzin is the effect of frictional resistance on the relative motion of earth and boom. However delicately the instrument is constructed, there must always be more or less friction;

and the aim of most constructors has been to minimize the friction as far as possible. Milne in his light form of horizontal pendulum uses the photographic method of taking the record; and in Rebeur-Paschwitz's very delicate instrument this method is also employed. What friction exists is therefore confined to the pivot or pivots, and to the resistance of the air acting on the boom.

The mechanical method of taking the record by the motion of light pointers over a strip of moving paper is used by the Italian observers in their vertical pendulums, and by Omori in his form of horizontal pendulum. This frictional resistance is believed to be practically negligible because of the massiveness of the steady body. The dynamical theory of the motion of the pendulum (either vertical or horizontal) is, however, rendered more complicated by the fact that the multiplication of the record is obtained by having the pendulum in touch with the earth at another point in addition to the necessary pivot and suspending wire. There is no doubt that Omori has with great ingenuity reduced the frictional resistance to a minimum under the conditions. Nevertheless the short end of the lever whose longer arm writes out the record must act impulsively on the horizontal pendulum; and only a very difficult piece of investigation could show to what extent this might influence the steadiness of a delicately suspended mass with a natural period of one minute.

It may be of interest to consider the magnitude of the force with which the writing lever acts upon the heavy body of Omori's horizontal pendulum. A diagram of the connexions is shown in Fig. 19. The heavy mass of the horizontal pendulum is represented by a circle $M$, in the centre of which is the delicately pivoted pin $A$ which fits the fork of the shorter end of the writing lever $ABC$. The axis, $B$, of the lever is fixed to the earth, and the further end $C$ presses on the revolving cylinder with a pressure of 5/3 milligrams or fully 1·6 dynes. With coefficient of friction 0·6 or 0·7 this will mean a transverse force of 1 or 1·1 dyne acting at $C$; and therefore a push of 10 or 11 dynes acting at $A$.

Suppose now that the boom with the mass $M$ is swinging with a period of 60 seconds. The force corresponding to displacement $x$ cms. is $(2\pi/60)^2 mx$ dynes, where $m$ is the mass in grammes. In Omori's larger form of instrument $m$ is about 14 kilogms. or 14,000 grammes. Hence for a displacement of 1 mm. or 0·1 cm. the force corresponding is $14,000/900 = 16$ dynes. Thus it appears that the push of the writing lever on the horizontal pendulum is quite comparable to the force involved in a displacement of one millimetre of the bob of the pendulum from its position of equilibrium.

In the portable form of his horizontal pendulum Omori uses a weight of 3 kilogms. with a natural period of oscillation of 20 seconds. This means greater stability in the proportion of about 2 to 1 with a corresponding diminution in

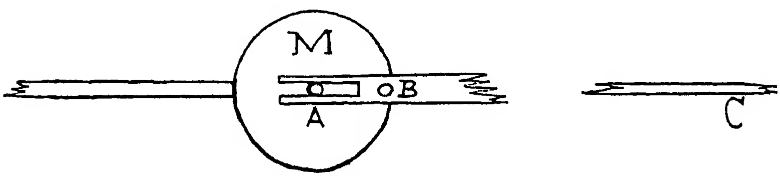



Fig. 19.

the relative effect of the frictional resistance due to the writing lever.

In his second memoir on the subject of seismometry Galitzin emphasizes strongly the well-known fact that under the influence of forced vibrations any system with a natural free period of oscillation cannot reproduce in its motions these forced vibrations either as regards amplitude or phase. He proceeds then to consider the nature of the problem when the horizontal pendulum is rendered aperiodic by the introduction of a suitable resisting force. This force he applies by means of an electromagnetic arrangement. Two copper strips are attached symmetrically to the end of the horizontal pendulum, one on each side. As the pendulum moves to and fro relatively to the earth each of these copper strips slips through between the poles of an electromagnet. This generates induction currents in the

copper strips and these by their reaction upon the magnetic field introduce a resisting force the strength of which may be varied through a large range by alteration of the strength of the electromagnets. The operator has thus the resisting force completely under control, and can adjust it so as to make the motion of the horizontal pendulum absolutely aperiodic. I quote a few sentences from Galitzin's memoir.

'The whole investigation shows quite clearly the advantage of using an aperiodic instead of a periodic instrument. The more complicated the true motion of the earth's surface the more complicated will be the corresponding seismogram, and the greater the difficulty of determining the march of the function which the displacement is of the time. But with a strongly aperiodic instrument the character of the seismogram closely approximates to that of the earth's motion.'

The powerful damping of the motion which is needed to render it aperiodic has, however, the disadvantage of diminishing the amplitude, and the usual methods of obtaining a record of weak earthquakes are inapplicable. Accordingly Galitzin devised an electric method, in which the relative movements of pendulum and earth were transformed into induction currents in a suitably adjusted coil moving with the pendulum. These induced currents were then passed through an aperiodic galvanometer, the deflections of which were recorded photographically on moving sensitized paper.

To test the theory as worked out Galitzin used the method already referred to of setting the instrument on a movable platform and comparing its records with the simultaneously recorded motions of the table. It is interesting to reproduce some of these comparisons.

The first diagram in Fig. 20 shows the free swing of the horizontal pendulum when the table is at rest. The remaining three diagrams and the three diagrams in Fig. 21 show what occurs when the table is set in simple harmonic motion. For ease of reference I shall refer to these by number from No. 1 to No. 7; and in the following table the free swing period, $T$, of the pendulum during each

experiment and the period, $T'$, of the imposed swing of the table or platform are given in contiguous rows.

| Period | Experiment | | | | | | |
|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
| $T$ | 10·77 | 10·77 | 10·77 | 10·6 | 10·6 | 8·82 | 8·82 |
| $T'$ | | 3·57 | 7·27 | 5·33 | 7·06 | 9·33 | irreg. |

Thus in experiment 2 the period of the platform motion was 3·57 seconds, while the natural period of swing of the horizontal pendulum was 10·77, or just about three times as long. The curves show that the pendulum reproduces

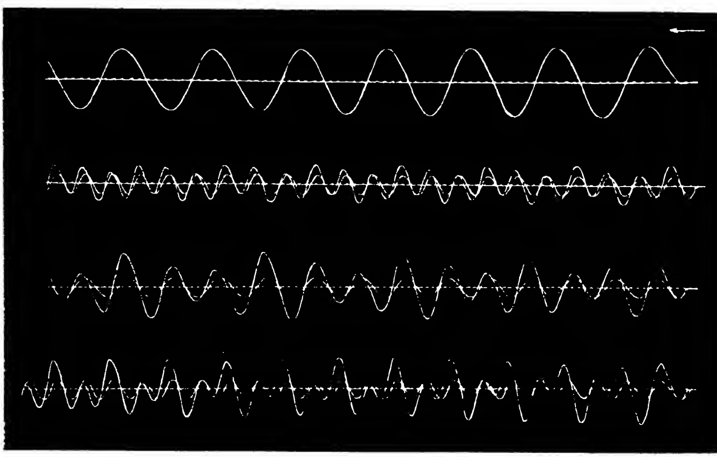


FIG. 20.

fairly well the motion of the platform; but there are evidences of accumulation of effect due to resonance. In experiment 3 the resonance effect is more pronounced, because the platform period is nearer in value to the pendulum period. They are nearly in the ratio of 2 to 3. Similar results are shown in experiment 4, in which the two periods are nearly as 1 to 2. In all these cases the platform curve is recognized by its absolute uniformity. On the other hand the motion imposed upon the pendulum reproduces the period, but the amplitudes are quite different.

Consider now experiments 5 and 6 whose curves are

shown in Fig. 21. In the one the ratio of the periods is almost exactly as 2 to 3; and in the other it is approximately as 17 to 18. It will be seen that the resonance is very marked. The pendulum still reproduces the period of the platform in its swinging; but the amplitudes produced are large, becoming larger the more closely the two periods approximate.

In experiment 7 we see the effect of an irregular and not even approximately periodic motion of the platform. As in the other cases the instrument begins with the same kind of movement as the platform, but because of the

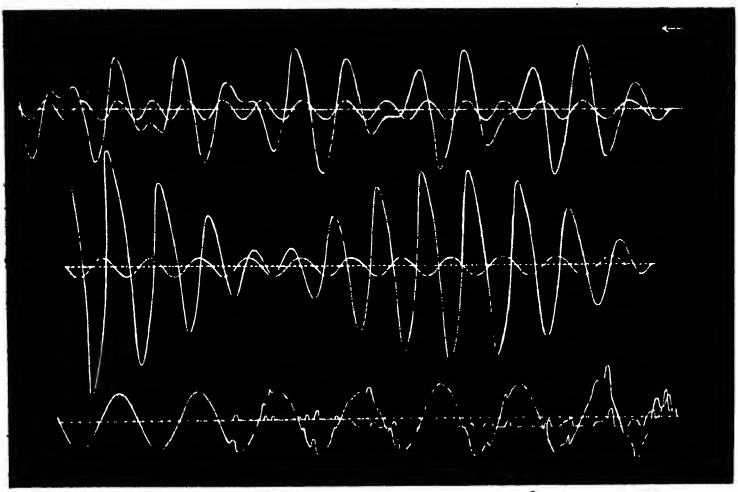


Fig. 21.

initial effect of a suitable succession of impulses it speedily acquires its own proper swinging motion, on which are superposed irregularities corresponding roughly with the irregular jerks of the platform. A slightly different succession of impulses at the beginning would no doubt have produced a very different amount of natural swing. It would be impossible to say beforehand what effect a particular succession of irregular impulses might produce.

We shall see below that the essential features of the seismograms of these artificial earthquakes are all to be met with in records of real earthquakes.

We pass now to the next set of curves shown in Fig. 22. These indicate the results obtained from experiments with the same instrument when its periodic oscillations are destroyed by means of sufficiently powerful damping. In other words the instrument is in an aperiodic state. The first three diagrams in the figure show the response of the aperiodic seismometer to sinusoidal movements of the platform with three different periods. The instrument curve begins on the left a shade later than the platform curve and indicates a certain irregularity for the first period. Thereafter the one curve is almost an exact copy of the

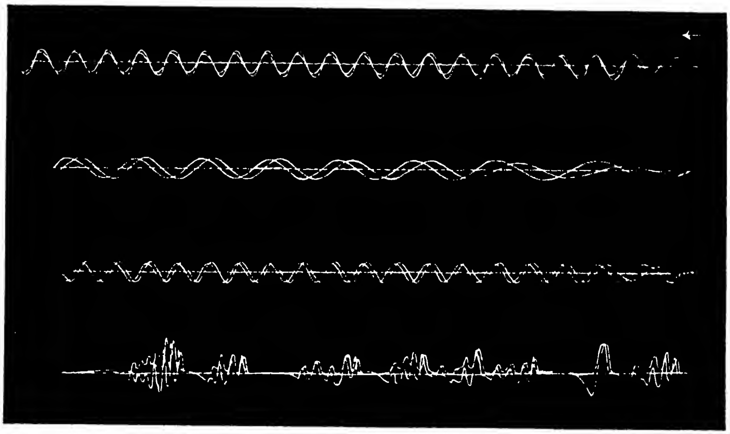


Fig. 22.

other, differing in phase and slightly in amplitude. In the last diagram of Fig. 22 we have an example of the manner in which an irregular disturbance of the platform is imitated on the seismometer. The platform curve may be easily distinguished as that in which there are straight line gaps during which the platform was at rest. During this resting stage the horizontal pendulum is seen to swing slowly and aperiodically from the displacement it happened to have towards its position of equilibrium. There is a correspondence between the succession of crests and troughs in the two curves, but there is not strict identity. Both in amplitude and phase there are considerable deviations.

Nevertheless there is no doubt that the aperiodic instrument reproduces something like the motion of the platform; whereas the periodic seismometer is absolutely untrustworthy in this respect, except for vibratory motions whose periods are distinctly smaller than the free period of the pendulum.

We may therefore conclude that in seismometers in which the frictional restraints have been reduced to a minimum only the more rapid movements of earth are approximately reproduced. When the frictional restraints are considerable, as they certainly are in the earlier types of pendulum bracket seismographs used by Ewing, Gray, and Milne, the record may not be very far from being a fairly faithful reproduction of the earthquake motion, indicating at all events the broad succession of motions and impulses. But in no case can we regard them as absolutely trustworthy in a truly quantitative sense.

In the analysis given above of the kinds of motion to which a horizontal pendulum would be sensitive a distinction was drawn between horizontal motion and tilting. It is difficult, as Milne has pointed out, to accept the fact of vertical motion and not at the same time to admit the existence of tilting. A measurable amount of tilting means a wave passing with a definite velocity over the surface of the ground. The maximum tilt associated with such a wave may not of course make itself wholly felt on the horizontal pendulum. The action is a kinetic, not a static one. And unless the period of the wave approximates to the period of free vibration of the pendulum the forced oscillation may have an amplitude smaller than the displacement which would be caused by a steady sustained tilt of the same amount. It is obvious, in fact, that a very rapid wave motion accompanied by tilting will have very little effect indeed upon the delicately poised horizontal pendulum, for the same reason that an ordinary galvanometer included in the secondary circuit of a rapidly working induction coil shows nothing but a quivering motion about its zero position.

Again, as clearly established by Galitzin, it is impossible

to separate dynamically the effects of the tilting from the effects of the horizontal acceleration.

Bearing these considerations in mind we cannot accept what at first sight may appear to be negative evidence as really disproving the existence of tilting. For example, Omori describes certain experiments with a set of horizontal pendulums, from which he concludes that in the ordinary moderate earthquakes which visit Tokyo there is no tilting. Two horizontal pendulums are set up side by side practically identical in all respects, except that the stability angle $i$, which is believed to determine the sensitiveness of the instrument, has different values in the different instruments. The smaller the stability angle $i$ which the vertical line makes with the line about which the horizontal pendulum swings, the less stable is the instrument, and the greater the deviation of the boom from its equilibrium position when a given *constant* tilt is imposed on the instrument about this equilibrium position. The argument is that if tilting be present to any appreciable extent the records of any earthquake obtained on the different pendulums will have markedly different amplitudes, the more sensitive giving the greater amplitude because of its increased response to the tilting action. The point is of some importance and calls for a detailed discussion. To make it quite intelligible to those whose mathematical knowledge is limited, I shall give the numerical details of four cases.[1]

The equation of motion of a body oscillating through small ranges about a position of equilibrium is

$$\ddot{x} + 2k\dot{x} + n^2 x = 0,$$

where $\ddot{x}$ is the acceleration due to the force of restitution urging the body back to the equilibrium position when its displacement from that position is $x$, where $2k\dot{x}$ is the acceleration due to resistances proportional to the speed $\dot{x}$, and where $n$ is a number proportional to the frequency of

[1] Wiechert in his *Theorie der automatischen Seismographen* (Abh. d. Kön. Ges. d. Wissen. zu Göttingen) gives a similar discussion, but not for quite the same object.

the unresisted vibration, being equal to the ratio of $2\pi$ to the periodic time. When $k$ is numerically smaller than $n$ the body has an oscillatory motion of period $2\pi/\sqrt{(n^2-k^2)}$. But when $k$ is the greater quantity there is no oscillation, the motion is aperiodic. The body when displaced from its position of equilibrium slowly settles back towards the equilibrium position, but does not oscillate to and fro about it. This aperiodic motion is produced evidently by making $k$ large enough. When $k$ is not too large, the motion is periodic. Under the influence of the frictional forces, the amplitude of the free vibrations will gradually diminish as time goes on, and unless the body receives a fresh impulse will ultimately decay completely. So much for the free vibration.

But now suppose the body to be acted on by an external rhythmic force such as would come into play if a suitable tilting of the ground took place below a horizontal pendulum. It is usual to represent this forced vibration by an expression of the form $f \cos pt$, where $2\pi/p$ is the periodic time of this sinusoidal forced vibration. The equation of motion then becomes

$$\ddot{x} + 2k\dot{x} + n^2 x = f \cos pt,$$

and the complete solution consists of a free vibration portion which decays in time and a forced vibration portion which persists so long as the forcing vibration continues to act. Ultimately this part is the more important; and its amplitude is measured by the expression

$$A^2 = \frac{f^2}{(1-n^2/p^2)^2 + 4k^2/p^2}.$$

In the horizontal pendulum the quantity $n^2 = gi/l$, where $i$ is the inclination to the vertical of the axis about which the pendulum rotates, $g$ is the acceleration due to gravity, and $l$ is the effective length of the boom and attached heavy body. This expression shows that the sensitiveness increases as the angle $i$ diminishes; and it can also be easily shown that the deviation of the pendulum from its natural position of equilibrium when the instrument is

tilted through a small angle $\phi$ is equal to the ratio $\phi/i$. Thus the smaller $i$ for a given tilt the greater the *statical* deviation.

But a consideration of the above expression for the amplitude when the tilting takes place with a rhythmic variation shows that there is no such simple relation between deviation and tilt at any given instant as might be expected from consideration of the statical effect only. For example, when $p$ is large compared to $n$, that is, when the forced vibration is quicker than the free vibration of the instrument, the term $(1-n^2/p^2)$ differs very little from unity, and the effect of $n^2$ is practically negligible. In other words we may increase $n$ from any small fraction of a given $p$ up to 1/3 of $p$, and the change in the amplitude will not thereby be affected by more than 10 or 12 per cent. But it is through $n^2$; which is proportional to the stability angle $i$, that the sensitiveness of the horizontal pendulum is believed to come into account. So long then as we are dealing with forced vibrations whose periods are less than one-half of the period of free vibration of the pendulum, we may vary the latter by varying $i$ through a wide range and not appreciably affect the amplitude of the motion imposed upon the pendulum.

To bring out the point quite clearly and at the same time to show how the value of $k$ influences the numerical details, I have calculated the relative values of the amplitudes for different ratios $n/p$ and for four different values of $k/n$. The results are shown in the following table. The first column contains the values of $n^2/p^2$, which for a given value of $p$ may be taken as proportional to the stability angle $i$, and therefore as inversely proportional to what is usually called the sensitiveness to tilting of the horizontal pendulum. The four following columns contain the values of the amplitudes of the vibration forced upon the horizontal pendulum in virtue of a periodic tilting movement of period $2\pi/p$ about the direction of the boom as axis. These are tabulated under headings which give the assumed values of $k^2/n^2$.

FORCED VIBRATION AMPLITUDES FOR VARIOUS VALUES OF THE PERIOD AND THE RESISTANCE.

| $n^2/p^2$ | $k^2/n^2 =$ 1/400 | 1/40 | 1/4 | 1 | Static Sensitiveness |
|---|---|---|---|---|---|
| 0·01 | 1 | 1·004 | 1·005 | 0·99 | 100 |
| ·04 | 1·04 | 1·04 | 1·02 | ·96 | 25 |
| ·16 | 1·19 | 1·18 | 1·08 | ·86 | 6·25 |
| ·36 | 1·56 | 1·5 | 1·14 | ·74 | 2·78 |
| ·64 | 2·71 | 2·27 | 1·15 | ·61 | 1·56 |
| ·81 | 4·76 | 2·92 | 1·09 | ·55 | 1·24 |
| 1 | 10 | 3·16 | 1 | ·50 | 1 |
| 1·21 | 4·22 | 2·46 | 0·89 | ·45 | ·83 |
| 1·44 | 2·19 | 1·72 | ·81 | ·41 | ·69 |
| 2·25 | 0·79 | 0·75 | ·51 | ·31 | ·44 |
| 4 | 0·33 | ·33 | ·28 | ·20 | ·25 |
| 9 | ·12 | ·12 | ·117 | ·10 | ·11 |

The last column of numbers gives the reciprocals of the quantities $n^2/p^2$ and shows what the relative deviations would have been had the statical law held. It is abundantly evident that the estimated sensitiveness of horizontal pendulums in terms of the time of free swing has absolutely no applicability to the practical case of the recording of earthquake motions. The significance of these calculations is shown clearly in their graphical representation (Fig. 23).

The quantities $n^2/p^2$ are measured horizontally, and the corresponding amplitudes vertically. If we suppose $p$ to be constant the abscissae will vary as $n^2$, becoming greater as the free swing period becomes less. This is the point of view to be taken when we are considering how the kinetic sensitiveness of the instrument depends on the stability angle $i$. Again, if we suppose $n$ to be constant, the abscissae will increase as $p$ diminishes, that is, the abscissae will vary directly as the square of the period of the forced vibration. This is the usual way of regarding the facts represented by the graphs. Taking the former point of view and regarding $p$ as constant, we see that the abscissae will be for the same instrument directly proportional to the stability angle $i$, so long as that angle remains small. The abscissae are therefore inversely proportional to the static sensitiveness, and this is shown by the dotted curve,

which is simply the last column of numbers plotted against the first. It is of course a rectangular hyperbola.

The full line curves represent the kinetic cases. For very slow periodic variations of the external force the kinetic case approaches the static case; for high values of $n^2/p^2$ the curves tend to coalescence. But as the period of the forced vibration is diminished towards equality with the natural period of swing of the pendulum the kinetic curves

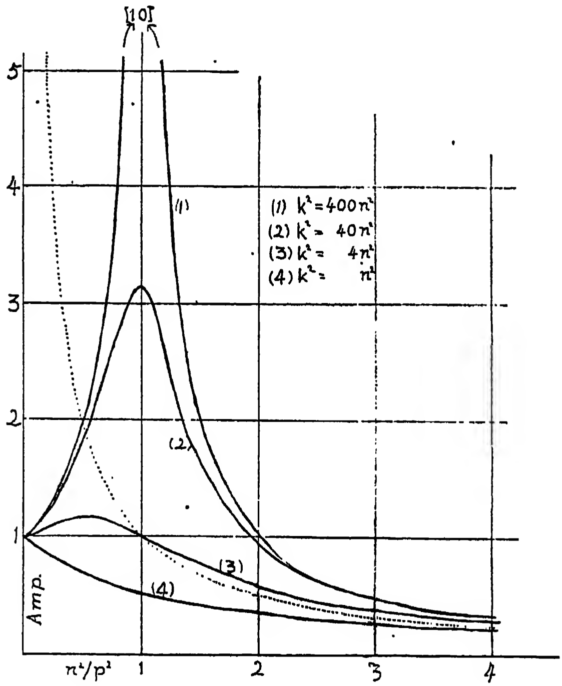



FIG. 23.

begin to deviate perceptibly from the static curve. In the two cases in which the frictional resistances are not large this deviation is very marked, and the sensitiveness of the instrument increases rapidly with the free swing period; but this increase ceases and the amplitude reaches a maximum when $n$ equals $p$, that is, when the forced vibration is isochronous with the free swing of the pendulum. Now in all practical cases the inventors have for good reasons

increased the period of the instrument as much as possible, retaining just sufficient stability to enable the horizontal pendulum to keep a steady position of equilibrium. Consequently the periods of the earthquake motions which have been found to occur in most cases are generally less and often very much less than the periods of free swing of the instrument. Thus in Omori's most delicate type of horizontal pendulum the free swing has a period of more than 60 seconds, whereas earthquake motions rarely exceed the half minute in period, and most of the periods are much smaller, such as 4, 8, 11, &c. In the bracket seismographs of Ewing, Gray, and Milne, the free swing period may be from 2 to 4 seconds; but in these the frictional resistances are considerable, and may prevent excessive resonance effects. In general, the longer the period in slow swinging instruments, the less prominent will resonance effects be; for there is less chance of the forced and free vibration periods being equal.

Professor Dyson and Mr. Heath have supplied me with data from which the necessary constants for the Milne horizontal pendulum installed at Edinburgh may be calculated. The natural period of swing was 15·3 seconds; and from a beautiful photographic record of the decaying motion when the instrument was deflected and then left to itself I was able to calculate the logarithmic decrement and from that determine the ratio $k^2/n^2$. Thus the ranges of motion for successive to-and-fro swings in the period 15·3 seconds were 9·6, 7·4, 6·15, 5·05. The ratios of the successive pairs are 1·3, 1·2, 1·23, which are as consistent as the method of measurement will allow. Taking the mean as 1·25 we find for the ratio $k^2/n^2$ almost exactly 1/800. By another method of measuring I found that the amplitude was diminished to one half in a distance of 1 mm. on the record. But 59 mm. correspond to 1 minute; hence working out by means of this relation I determined the ratio of $k^2/n^2$ to be 1/1300. It is not certain that at the time the record of the decaying motion was taken the period was exactly as stated; and in any case the smallness of the record prevents any great precision in the measure-

ments either of time or amplitude. We shall not be far wrong if we take 1/1000 to be about the value of the ratio of $k^2/n^2$ for the particular seismometer in use in the Edinburgh Royal Observatory. A consideration of the expression above shows that except in the immediate vicinity of $p = n$ the effect of taking $k^2/n^2$ smaller than 1/400 is of no great consequence.

I have no information as to the relative magnitudes of the frictional resistances in other cases, although they could be easily obtained by noting the rate of decay of the free swing. Provisionally we may take the first case ($n^2 = 400k^2$) as corresponding to Omori's horizontal pendulum, and the second case ($n^2 = 40k^2$) as corresponding to the more stable type of bracket seismograph. In both cases in accordance with the law of resonance a maximum is reached when the free and forced vibrations have equal periods; and the sensitiveness in the neighbourhood of this condition is greater than would be expected from the statical law. But thereafter as the period of the forced vibration is diminished, or as the period of the free vibration is increased, the amplitude falls off towards a definite value for excessively rapid forced vibrations or for extremely long free vibration periods. The law of change of sensitiveness as estimated by the amplitude imposed upon the instrumental record—and there is no other real way of estimating sensitiveness—follows a law entirely different from that which the statical relation between deviation and tilt would suggest. The sensitiveness in the kinetic cases diminishes when it ought to increase according to the static law. In the third curve, which represents a case of great frictional resistance but not quite great enough to destroy periodic motion of the pendulum, the law of sensitiveness follows the static law very closely up to the critical condition $n^2 = p^2$; but for smaller values of the ratio $n^2/p^2$ there is hardly any appreciable change. Finally, for the limiting aperiodic case the sensitiveness increases steadily but slowly all the way as $n^2/p^2$ diminishes towards zero. It is easily seen from the formula that $A$ is inversely as $(1 + n^2/p^2)$, so that there is no critical condition given when $n = p$.

In this discussion I have assumed the usual results of forced vibrations acting upon a vibrating system with a certain amount of frictional resistance present. But Galitzin's curves show that this theory is incomplete. Theoretically the free vibration which may be started at the beginning is regarded as decaying in time so as to become comparatively unimportant, while the forced vibration is sustained. This solution might be called the kinematic solution of the problem; but dynamically it is incomplete. There seems to be little doubt that the free vibration is always being started anew by appropriate impulses from the forced vibration. If the action of the forced vibration were to cease at any instant the system would continue to swing to and fro with the free-swing period. This tendency to start free vibrations is always present; and will be aided or thwarted according to the momentary phase of the forced vibration. The net result seems to be that although the free vibration may not always show itself distinctly it is always present in an incipient state. There will consequently be interference effects between the forced vibration imposed on the system and the more or less incipient free vibration which may be said to be potentially present.

This discussion proves four points of some importance.

(1) The amplitudes of records obtained on delicate seismometers in which frictional resistances have been reduced as much as possible have no simple relation to what might be called the statically estimated sensitiveness of the instrument. The amplitudes depend far more upon the relative amounts of friction present and upon the numerical relation between the periods of the forced earthquake vibrations and the free-swing period of the instrument.

(2) The conclusion that tilting in earthquake movements is disproved by the fact that two seismometers of different sensitiveness *as estimated statically* do not give proportionally different amplitudes in their records of the same earthquake is dynamically unsound.

(3) When with a horizontal pendulum swinging fairly freely the period of the earthquake motion approaches

within, say, 30 per cent. of the period of free swing of the pendulum, the amplitude of the record is in virtue of the law of resonance greatly increased. In such cases, and they are of frequent occurrence, the recorded amplitudes give no real measure of the earthquake amplitudes. This is beautifully illustrated by Galitzin's curves.

(4) Even when the pendulum is made aperiodic, the only condition in which according to Galitzin's investigations anything like a faithful reproduction of the earth's motion is possible for all periods, the increase of the static sensitiveness does not greatly increase the kinetic sensitiveness.

Bearing in mind these conclusions we are ready to consider the significance of the records which have been obtained on seismometers and seismographs of various kinds.

First, as regards the evidence of pronounced resonance, I shall quote from a letter which I sent to *Nature* (vol. xli, p. 32) in reference to the earthquake which occurred in Tokyo on April 18, 1889.

'It was my good fortune on the day in question to be engaged in conversation with Professor Sekiya in the Seismological Laboratory (of the Imperial University of Japan) at the very instant the earthquake occurred. We at once rushed to the room where the self-recording instruments lay, and there, for the first time in our experience, had the delight of viewing the pointers mark out their sinuous curves on the revolving plates and cylinders. At first sight it seemed as if the pointers had gone mad, tracing out sinuosities of amplitude five or six times greater than the greatest that had ever before been recorded in Tokyo. There was not much sensation of an earthquake; indeed, after the first slight tremor that attracted our attention, we felt nothing at all, although in the irregular oscillations of the seismograph pointers we had evidence enough that an earthquake was passing. Very few in Tokyo were aware that there had been an earthquake until they read the report of it in next day's papers.'

A similar experience is thus described by Milne in the account of what he observed in Tokyo on the morning of October 28, 1891, when the great Mino-Owari earthquake wrought such havoc in central Japan :—

'From the manner in which my house was creaking

and the pictures swinging and flapping on the wall I knew the motion was large. My first thoughts were to see the seismographs at work; so I went to the earthquake room, where to steady myself I leaned against the side of the stone table, and for about two minutes watched the movements of the instruments. It was clear that the heavy masses suspended as horizontal pendulums were not behaving as steady points, but that they were tilted first to the right and then to the left . . . That whenever vertical motion is recorded there must be tilting and therefore no form of horizontal pendulum is likely to record horizontal motion, is a view I have often expressed. What I then saw convinced me that such views were correct.'

So far I agree with Milne that vertical motion implies tilting; but I am not so sure that tilting is, except in certain extreme cases, so conspicuous as to mask the effects of to-and-fro horizontal motion. As Galitzin has shown, no horizontal pendulum can separate out the effects of tilting and of horizontal motion. But we do not require to invoke the existence of tilting to explain these great excursions of the seismograph pointers. Pronounced resonance effects will exist when the earthquake motion is fairly rhythmic with a period not very different from the period of free swing of the seismometer. In the earthquakes just described there were periodic motions which fairly well synchronized with the free-swing period of the instrument. In the earthquake of April 18, 1889, the motion was comparatively gentle with a long period of several seconds. In the earthquake of October 28, 1891, on the other hand, there was mingled with the slower periodic motions, as experienced at Tokyo, an alarming amount of rapid oscillations. The position of the epicentre of the latter earthquake was known only too well; but it was not possible to locate the origin of the earlier shock with any precision. Probably it had its source below the waters of the Pacific Ocean to the south or south-east of Tokyo. The shock of April, 1889, is, however, of special historic interest, as being the first earthquake which was recognized as having produced a characteristic effect on the delicate horizontal pendulums installed by Rebeur-Paschwitz at Potsdam and Wilhelmshaven. See below, chapter xi.

It is interesting to compare seismograms which the same

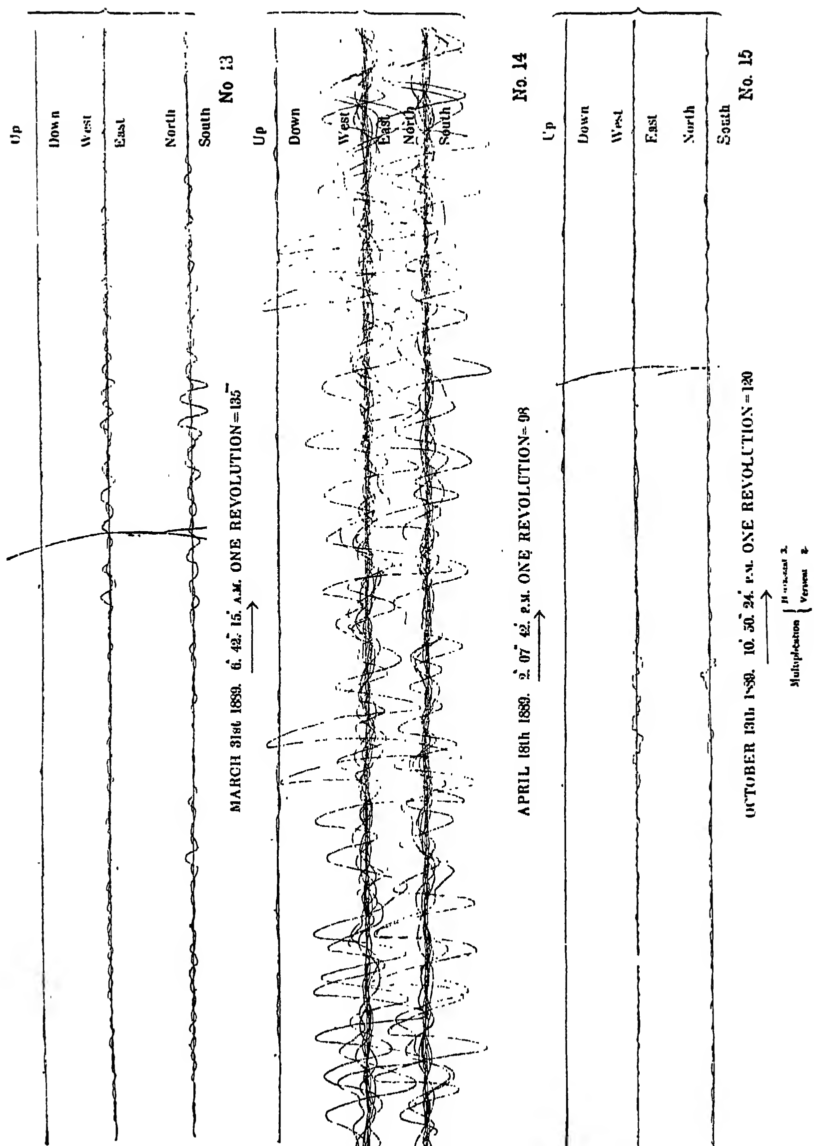



FIG. 24.

instrument gives of different earthquakes. In Fig. 24

I have reproduced on a reduced scale of 4 to 11 three sets of diagrams obtained with an early form of the Gray-Milne seismograph. The middle set contains the three component seismograms of the earthquake of April 18, 1889, already described, while the other two sets show the same elements for other earthquakes which occurred the same year. In all the vertical motion is small; and although it may reach a larger amplitude in the last of the three, yet it is on the whole more pronounced in the other two. There is not, however, much to choose between the vertical seismograms of the first and second, unless it be that there is in the latter a clearer indication of long oscillations of periods of from 3 to 4 seconds. It is in the horizontal seismograms that the startling difference is shown. The conclusion seems to be obvious. In the April earthquake record we are face to face with a real resonance effect, the period of the motion of the ground being sufficiently in tune with the natural time of oscillation of the horizontal pendulums to start it swinging with that period. For if it were simply a question of tilting, and if tilting be a necessary concomitant of vertical motion, we should have expected the horizontal seismograms of the March earthquake to show as large amplitudes as those of the April shock. There is no doubt at any rate that the character of the horizontal seismograms is entirely changed when we pass from the records for the April earthquake to those of the other two.

In the later forms of the Gray-Milne seismometer the records are traced on a strip of paper drawn over the cylinder at a steady rate which is suddenly accelerated when the earthquake begins. In Fig. 25 a portion of a set of seismograms is shown, reduced in the ratio of 10 to 22 from the diagram in Baron Kikuchi's account of recent seismological investigations in Japan. This indicates in my opinion in an unmistakable manner the effect of resonance upon the character of the record. The portions shown are for an interval of fully twenty seconds. In the early part of each seismogram rapid vibrations are evident especially in the vertical component. Very soon, however, a comparatively long period oscillation begins to appear in the East-West

seismogram. On this the rapid corrugations are superposed. In the fourth second this seismogram breaks off but begins again in the eighth second, and after ten seconds

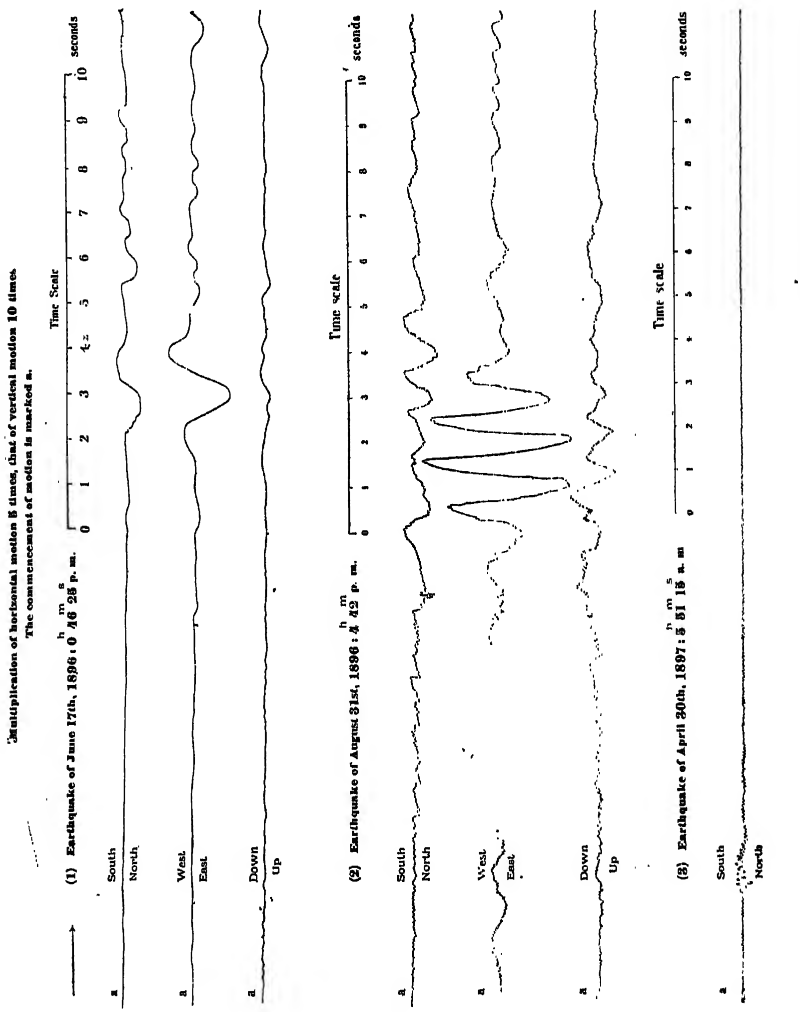



FIG. 25.

makes several large oscillations in which the rapid corrugations are visible only at the crests and troughs, being quite masked when the movement is swift from side to side.

Similar oscillations, not so strongly marked, are seen on the north-south and on the vertical motion seismograms.

There seems to be little doubt that we are dealing here with instrumental peculiarities and not with true measurements of surface motions. The periods of the earthquake motions are probably fairly well indicated; but not the amplitudes. This indeed is impossible with free-swinging horizontal pendulums, except for very rapid oscillations, such as generally show themselves at the beginning of a shock. As illustrated by the third curve in Fig. 22 an arbitrary impulse may start the pendulum moving with its own period, and on these larger movements of the seismograph rapid oscillations are frequently superposed. These we may regard as true earthquake movements; but we cannot in the light of the preceding discussion consider the large excursions of the recording pointers as reproducing the real motion of the ground.

The elimination of periodicity in a seismometer must be accomplished by the introduction of a suitable 'damping' arrangement under complete control, either electromagnetically (after Galitzin) or by pistons working in perforated cylinders through which air can pass, as in Wiechert's forms of instrument. An accurate reproduction of the motion of the ground is possible only when the aperiodic condition is fulfilled; but this is difficult to realize practically because of the accompanying diminution of amplitude. Even with Wiechert's so-called astatic instruments and others of modern construction provided with a controllable damping arrangement there is still periodicity, and the records show resonance effects similar to those we have just been discussing. Nevertheless suitable damping diminishes the magnitude of the resonance effect and improves the action of the seismograph.

# CHAPTER VI

## EARTHQUAKE DISTRIBUTION

Seismic and Aseismic Regions. Definition of Seismicity. Survey of Limited Areas. Milne's Survey of Tokyo. Earthquake Catalogues. De Ballore's Methods. Rudolph's Analysis of Sea-quakes. Milne's Chart of Large Earthquakes. De Ballore's Criticisms.

It has long been recognized that certain regions of the earth's surface are peculiarly subject to earthquakes; and that in certain other districts earthquakes rarely if ever happen. Between these extremes there are all possible degrees of earthquake visitation.

It may be laid down as a general rule, that where small and moderate seismic shocks are frequent, destructive earthquakes are more familiar than in regions not subject to frequent shakings. There are of course exceptions, such as at Comrie in Scotland, where no destructive shock has happened in historic times, or at Lisbon, famous in history because of the great earthquake of Nov. 1, 1755, and yet not a place of seismic importance in the ordinary sense of the term. Nevertheless it is generally true that destructive earthquakes visit at intervals regions which are characterized by great frequency of slight or moderate shocks.

We may use the term seismic frequency as an indication of the degree to which a given region is subject to earth shakings. We may suppose the number of individual shocks to be known as having occurred over a limited area in a given number of years. The ratio of these two numbers will give an average annual seismic frequency for that region during that time. The greater the number of years taken into account, and the more care in noting and recording the shocks, the better will be the approximation to the estimate of the seismic frequency. Unfortunately the regular recording of small shocks is comparatively recent, so that statistics for great stretches of the earth's surface are necessarily far from complete. As an illustration take

the case of Japan. We may suppose the number of earthquakes recorded, say, in twenty years to be divided by 20, giving a good annual mean. Then, dividing this annual mean by the area of the country, we get the mean annual frequency per unit area. Let us call this the seismicity of the country. Now, there are many districts in Japan which are hardly ever visited by perceptible shocks ; and the vast majority of recorded earthquakes occur in a few limited districts. It is clear then that the seismicity estimated as above for the whole country must be smaller, probably much smaller, than the true seismicity of any one of these limited regions. Indeed just as we recognize, broadly speaking, certain countries as of seismic character, so in any one of these countries we recognize great differences in the earthquake frequencies characterizing different parts of the country. Milne in his catalogue of 8,331 earthquakes which happened in Japan between the years 1885 and 1892 concludes that there are fifteen distinct districts susceptible to seismic effects ; and that outside these the rest of Japan is practically quiescent.

Not only so, but in the limited region of the city of Tokyo the susceptibility to earthquake shocks varies greatly with locality. This question was investigated in 1887–8 by Milne by means of a system of postcards distributed to fully 100 observers resident in different parts of the city. When a shock was felt the observer filled in the information on a postcard and returned it to Professor Milne. During the six months from November 15 to May 5, 496 records from 103 observers were sent in. Of these 370 came from 61 observers living on high ground in the west and north of Tokyo ; and 126 from 42 observers living on low ground. Note that we are dealing with the susceptibility of observers to earthquake shocks, not with instrumental records of earthquakes. The distinction is important and should be borne in mind in all questions based on the statistics of earthquakes felt or recorded by man.

It would thus appear that in Tokyo more shocks are felt by observers on high than by observers on low ground—about twice as many, indeed. The reason of this is to be

found in the following further facts. The records had to do with 69 distinct earthquakes. Of these 36 were felt over a wide area outside Tokyo; but of these only 6 were felt all over Tokyo. The remaining 33 were felt on the high hilly ground only. An examination of the periods of vibration of these various shocks as recorded on the instruments in Tokyo gave an average period of 0·76 for the 6 felt all over Tokyo, and 1·85 for the 30 felt only on the high ground. Thus it would seem that for a moderate shock to be felt by an observer on low alluvial ground, a quicker vibration is necessary than when the observer is on high hilly ground. In connexion with this it is well also to mention that the instrumental record shows in general a larger slower motion on the low alluvium than on the rocky ridge. It is apparent, in fact, that extent of movement does not entirely determine susceptibility to an earthquake. The rapidity of the vibration is also an important factor. The hearing of earthquake sounds without the sensation of movement is, in fact, simply a limiting case, the vibration being very short in period but small in amplitude.

The early catalogues of earthquakes prepared by Mallet, Perrey and others, though necessarily incomplete, indicated the broad features of seismic distribution over the surface of the earth; and within recent years the industry of M. de Montessus de Ballore has given a greater certainty to the conclusions which had been indicated if not expressly formulated. De Ballore has published his own views in an important work, *Les Tremblements de Terre, Geographie seismologique*, which is essentially a detailed account of all the earthquake regions of our globe.

Some years ago de Ballore introduced a method of measuring seismicity, practically identical with the method of finding the average annual frequency of shocks per unit area. In the book just published he has definitely given up this method on the ground that earthquakes are discontinuous phenomena, so that all attempts to represent seismic distribution over the world by means of continuous seismic lines or shaded areas rest on a false and unscientific

basis. He has accordingly used in his book a discontinuous method of indicating seismic frequency. A small black disk is set over each locality, the radius of the disk indicating on a purely conventional scale the average annual seismic frequency. The appearance of his charts is very striking, and the eye is able to pick out at once the places of greatest seismicity. Probably the method is as good as any other in the case of districts only moderately subject to seismic disturbances. But in countries like Italy and Japan where shocks are numerous there is not the same difficulty in drawing lines of equal seismicity with fair accuracy.

The accuracy of earthquake statistics must depend ultimately upon the amount of the earth's surface inhabited by civilized man. There are many large tracts of land destitute of inhabitants, or inhabited by nomadic or semi-barbarian races. Any shocks occurring there are not recorded, unless they happen to be very severe ones which cause terror and panic.

Again, the land surface of the globe is only a fraction of the whole surface; and it is obvious that many earthquakes originating below the sea will be unfelt by dwellers on the nearest land. As a matter of fact we know that a large percentage of shocks which are felt have their source beneath the neighbouring bed of ocean. For example, almost all the moderate quakes in the Tokyo-Yokohama district of Japan originate in the Pacific ocean to the south and east. This no doubt is why they are as a rule so moderate in their intensity. It is clear then that an earthquake statistic based wholly on felt shocks cannot but be incomplete.

This incompleteness has been partially filled in by Rudolph's tabulation of the so-called sea-quakes; but still more effectively by Milne's recent charts of large earthquakes 'which shake the whole earth'.

The sea-quakes tabulated and discussed by Rudolph differ from ordinary earthquakes only in the fact that their origins have been below the ocean, and that generally they have not been powerful enough to make themselves felt in the nearest countries. The disturbance produced over

a limited region of the ocean bed has caused condensational waves to pass upward through the water. These have been felt and heard by passing vessels; and it is from the logs of the captains that Rudolph drew his information. It is obvious, however, that the record of shocks felt by ships at sea must be far more incomplete than the records of shocks on land. The ships are comparatively few in number compared to the great stretches of ocean over which they navigate. Moreover the routes are mainly determined by commercial considerations; and large parts of the sea are rarely if ever visited. Very many suboceanic shocks must escape detection.

The nature of the shock experienced on board ship is best indicated by quotation from some of the logs collected and discussed by Rudolph.

'9 June, 1882. Capt. Stiven of the *Arethusa*, 32° 41′ N., 39° 50′ W. Experienced sharp shock of earthquake. It shook the ship so violently from stem to stern that all hands came running out to see what was the matter. I was standing on the poop at the time and the vibration seemed vertical. The wheel was shaken in the helmsman's hands. It was accompanied by a rumbling noise like distant thunder, but seemed close to us. It was something like a heavy cask being rolled quickly along the deck from forward, and in fact I thought at first it was so, but a moment's consideration showed it could not be, the ship shaking far too much for that . . . The phenomenon lasted for say 10 seconds.' (Rudolph estimates intensity at VII.)

'22 Dec., 1884. The Azores. Capt. Balderston of the *Belfast*, 34° 34′ N., 19° 19′ W. The ship was shaken by an earthquake which lasted from about 75 to 90 seconds. The shaking was accompanied by a loud rumbling noise which as heard from the cabin resembled the sound which would be made by the rolling of large empty or iron tanks about the decks; but which as heard from the upper deck and in the open air was as that of not very distant thunder, and it appeared to fill the whole air . . . I cannot say in what compass bearing of the visible sky it commenced, but it travelled rapidly through the air towards the S.W. . . . The helmsman found the steering wheel much shaken as he held it, and in the cabins and cookhouse tin ware, crockery ware, and other light articles were rattled about . . .' (Estimated intensity VI.)

'9 May, 1877. St. Paul's Islands. Capt. Murdoch of the *Denbighshire*, 0° 52′ N., 28° 18′ W. Two severe shocks of earthquakes: the first shock was like a jarring of everything in the ship. On deck it appeared as if the chain cables were running out and the topmast yards coming down by the run, and it seemed as if at every step we took on deck we must fall down: this shock lasted 30 or 40 seconds. The water was not at all agitated nor phosphorescent, the weather was rather sultry and hot below, so that I had been on deck most of the watch with the mate. After the first shock was over I told the carpenter to sound the pumps. All hands had rushed on deck, thinking the ship was on shore, and while sounding the pump the second shock occurred. It was sharp and instantaneous, as if a large cannon had been fired immediately below the ship. There was no mistaking this last; it was a volcanic eruption or explosion. The noise that accompanied the first shock was like the low groaning of distant thunder, but yet it appeared near and about us. The sensation and noise during the first shock was most peculiar. I tried the temperature and specific gravity immediately after the second shock, but there was no apparent change.' (Estimated intensity VII.)

There is no mistake as to the general nature of these experiences, and the explanation of all the phenomena is simple enough. As pointed out in chapter x below, a disturbance originating below the bed of the ocean will, whatever may be the angle of incidence within the rock, proceed through the water as a compressional wave in a direction almost perpendicular to the general lie of the ocean bed. This is because the speed of propagation of the condensational wave in water is much smaller than the speed of propagation of the elastic waves in the rock. For the same reason a second refraction at the upper surface of the water into the air will give rise to waves of condensation in the air passing upwards in a direction almost truly vertical. These compressional waves, if of sufficiently short period, will produce sound. The hearer will hear this sound as being all around him, emerging as it does almost vertically. He will not hear it first as coming from any direction, but he may hear it in the later stages coming from the direction in which the later disturbances are passing

away through the water, that is, in the direction opposite to that from which the disturbance originally came. The shaking of the ship from stem to stern and the jarring of everything in the ship are just what a complex succession of variations of pressure in the water would produce through the solid and almost rigid body of the ship.

The general results of Rudolph's investigation are given in the following table of sea-quakes from 1720 to 1886.

| Sea | Number of shocks |
|---|---|
| N. Atlantic | 52 |
| Eq. Atlantic | 65 |
| S. Atlantic | 10 |
| W. Indies, Caribbean Sea | 34 |
| Mediterranean | 31 |
| Indian Ocean | 28 |
| E. of N. Pacific | 22 |
| W. of N. Pacific | 14 |
| E. of S. Pacific | 47 |
| W. of S. Pacific | 10 |
| E. Indies | 20 |
| Total | 333 |

Remembering that there must be a tendency for more earthquakes to be observed in this way in regions frequented by ships than in regions hardly ever visited, we must conclude that the numbers are too few to serve as a basis for sure argument, with perhaps two exceptions. These are the equatorial Atlantic and the East of the South Pacific. The shocks felt in these seas are so numerous compared with the shocks felt in more frequented seas that we may safely regard the equatorial Atlantic and the East of the North Pacific as characterized by a comparatively large seismicity.

Regarding Milne's charts referred to above we shall have more to say in chapter xii. It will suffice here to mention that the evidence is derived from the seismograms of unfelt earthquakes obtained from more than forty stations widely distributed over the earth's surface. From a comparison of the records of any one world-shaking earthquake, it is possible to fix within certain limits the position of the epicentre. I reproduce the last chart prepared by Milne and presented in the annual report to the British

Association which met in Leicester in August, 1907. The origins are found to lie within the various oval regions

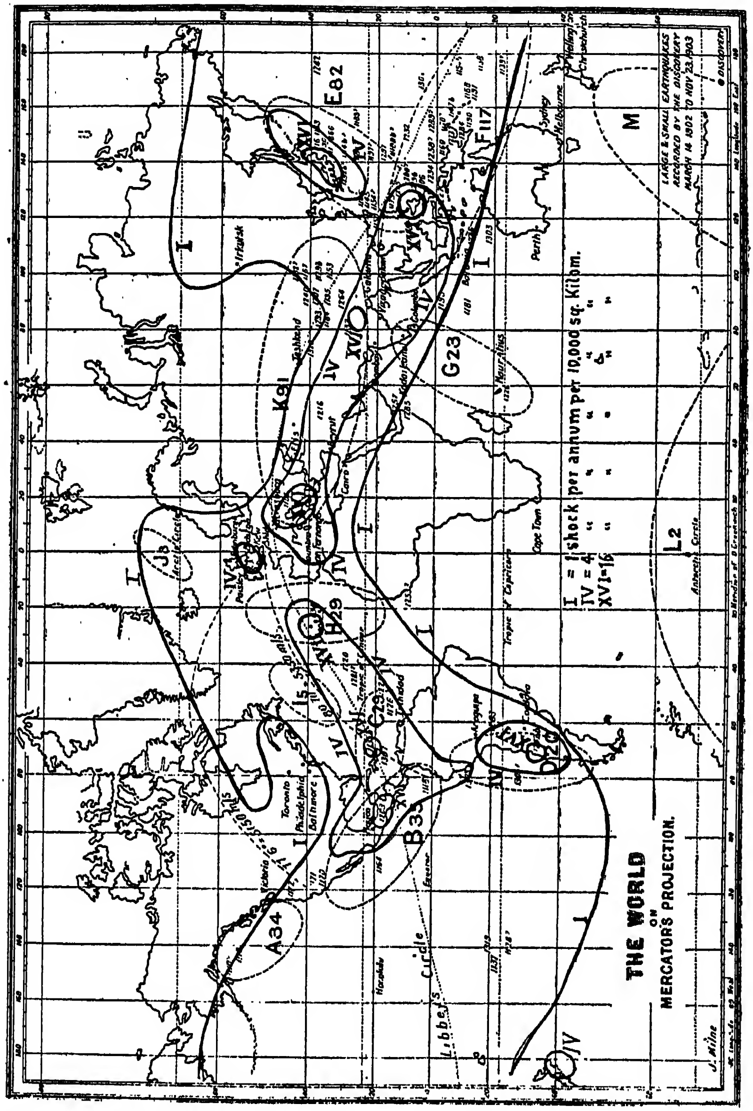



FIG. 26. The dotted ovals are Milne's seismic districts. The full lines are drawn in accordance with de Ballore's statistics.

marked off by dotted lines. The small type numbers

represent the individual earthquakes of which instrumental records were obtained practically from all the stations indicated on the chart. The number is marked in the approximate epicentre of the corresponding earthquake, and is the tabulated number of the shock according to Milne's own catalogue at Shide, Isle of Wight. The different districts are named by the letters of the alphabet from *A* to *M*; and the large type numbers give the total number of earthquakes which had their origins in the corresponding region between the years 1899 and 1905, the interval of time over which this method of determination has been available.

De Ballore has criticized this method on several grounds, the most important being that Milne's charts indicate origins to be in regions which ordinary earthquake statistics show to be aseismic, and that certain well-known seismic areas do not appear on Milne's charts at all. This argument, however, loses a good deal of its force when we realize that there are destructive earthquakes which do not shake the whole earth. They originate near the surface, are of the kind known as volcanic shocks, not tectonic, and have therefore a very evident effect in epicentral regions; but they are not intrinsically intense enough to send measurable disturbances over the whole earth. The destructive character of an earthquake as experienced at the epicentre depends directly on its intrinsic intensity and inversely on its depth. A deep-seated but powerful disturbance might have less destructive effect at the epicentre than a shallow but much less powerful disturbance. The former would be recorded on delicate seismographs all over the earth's surface, while the latter might affect delicate instruments only within a comparatively limited area.

We have, in short, no right to expect absolute agreement between the two ways of seismically surveying the globe. The one method represented by de Ballore's statistics takes into account only episeismic effects strong enough to be recognized in peopled districts. The other method deals with tremors which have passed through the earth's substance, and over the whole circumferential surface.

To bring out the differences and similarities I have entered in full lines on Milne's chart the general results of de Ballore's tables of seismicities brought down to the year 1897 (see Gerland's *Beiträge zur Geophysik*, vol. iv). The lines of equal seismicities marked I, IV, XVI show the localities where the seismicities are respectively 1, 4, and 16 shocks per annum per 10,000 square kilometres ( = 3861 sq. miles).

It will be seen that Milne's districts *B*, *C*, *D*, *E*, *F*, *K* agree closely with de Ballore's statistics and that *A*, *H* have a good deal in common. We could not expect the antarctic districts *L* and *M* to give any direct statistics. Thus *I*, *J*, and *G* are the only ones which seem to lie outside the seismic areas given by de Ballore's statistics. *I* and *J* have a very small number of earthquakes associated with them and are evidently of little importance; so that *G* in the Indian Ocean is the only important district which finds no place in de Ballore's tables. It is to be noted that this district is almost entirely oceanic; and that many earthquakes originating within it would be very feebly felt in neighbouring countries.

A well-known earthquake region is in European Italy and the Alps, having according to de Ballore a seismicity equal to that of Japan. This in Milne's chart is a mere part of an extensive district, which as a whole is not much more seismically active than the region *E* including Japan. Thus Italy and the Alps do not constitute a region specially visited by world-shaking earthquakes. Many of the violent Italian shocks are indeed of comparatively small extent, not being recorded on delicate seismographs at distant stations.

Taking a broad view of earthquake distribution we see that the strongly marked seismic regions are situated on the borders of continents and in regions where we know geological changes to be in progress. Thus all round the Pacific Ocean there is a succession of seismic areas, beginning with the East Indies and passing north and east by way of the Philippines, Japan and Alaska, and then south again along the west coast of the two Americas. Another well-

marked region begins in the west of Europe and passes east through Sicily, Italy, Greece, Syria, India, and Central Asia. The West Indies constitute a district which may be taken along with Central America. In these regions are contained practically all the known localities subject to pronounced seismic activity. The recent catastrophes at San Francisco, Valparaiso, Jamaica, and Sumatra are all good illustrations.

As already indicated in chapter i, the prime cause of earthquakes is the instability of the earth's crust. This instability through geological time is demonstrated by the foldings and faults which occur in endless variety; and is connected with the heterogeneity of the crust and with the interplay of gravitational and elastic forces. The problem to be considered is not, why do earthquakes occur, but why do they favour particular portions of the earth's surface? The only answer to this question is that we have to do with a fundamental feature of the earth itself. The broad distribution of heights and depths, of land and water, must be accepted as a condition whose lines were laid down in the remote past, and whose development has been the natural result of the interplay of gravitational, cohesive, and elastic forces in a material crust of definite heterogeneity. Given a particular distribution we may be able to follow out the changes which these forces necessarily compel; but some initial heterogeneity we must assume. There is no getting behind this fundamental fact.

# CHAPTER VII

## PERIODICITIES

Earthquake Frequency. Possible Causes. Tidal Stresses due to Sun and Moon. Annual and Semi-annual Periodicities. Method of Analysis. Rayleigh and Schuster's Application of Theory of Probabilities. Expectancy in Hap-hazard events. Author's original Investigations. Davison's corroborative work. Annual Frequency in Northern and Southern Hemisphere. Possible Meteorological Causes. Slow Barometric changes. Omori's Discussion of Japan Earthquakes. Criticisms and Conclusions. Rainfall, Snowfall, Denudation, Deposition.

In the preceding chapter we have seen that the prime cause of earthquakes is the heterogeneity of the earth's crust combined with an approximate state of equilibrium or isostasy which is constantly breaking down under readjustments. In regions where shocks are frequent the material of the crust is in what might be called a seismically sensitive state; and it is conceivable that under the influence of external periodic forces the crust may yield in a similar periodic manner. This consideration is the scientific foundation for the many attempts which have been made to trace possible periodicities in earthquake frequencies in various districts, small or large.

In 1885 I communicated a paper on Earthquake Frequency to the *Transactions of the Seismological Society of Japan;* a Society which ceased to exist in 1892, and of which the Transactions are not readily accessible outside seismological observatories and certain scientific societies. For these reasons I propose to quote with slight verbal changes certain introductory paragraphs.

'Earthquake Frequency depends on two distinct things—the seismic sensitiveness of the region, and the existence at the proper time of a stress suitably applied.

'Now if there is any marked periodicity in earthquake frequency, there must be a corresponding periodicity in either or both of these two independent factors. It is possible of course that there may be a natural period in the

growth of the sensitiveness of a given region from one time of yielding to that degree of sensitiveness necessary for the next; but it is difficult to believe that such a periodicity would be generally characteristic of all regions, or of such purely terrestrial causes as have been specified. Since our object is to discuss periodicity as displayed in earthquake frequency over the whole globe, we may confine our attention simply to the various cosmic and meteorological phenomena which may possibly influence seismic activity. In so confining our attention, however, we must not be understood to say that these are the causes of earthquakes, but merely that they are possible determining factors in earthquake frequency.

'Of all bodies external to the earth the sun and moon alone can be expected to produce any appreciable effect in virtue of direct gravitational action. The idea that seismic or volcanic phenomena are due to tidal actions produced in the interior parts of the earth is no new one. In the early days of geology, when the earth's solid crust was supposed to enclose a molten interior, such an idea was a very natural one. With this theory as a guide, geologists tried to find some relation between the moon's altitude and the intensity of volcanic eruption, but with small success. In 1839, the theory of the liquid interior enclosed by a thin solid shell received its death-blow at the hands of Hopkins, who showed that such a structure was quite incompatible with the astronomical phenomena of precession and nutation. Not only would the tides produced in the fluid interior utterly do away with precession as it exists; but the thin crust would have to be of infinite rigidity to be able itself to resist the deforming tidal action. That precession and nutation might be as they are, the whole crust, supposed rigid, would have to be at least 800 miles thick. In 1862, Sir William Thomson [afterwards Lord Kelvin] followed up Hopkins's researches by a discussion of the problem of the yielding of the earth to tidal action. That it must yield to some extent is indisputable, since certainly its rigidity is not infinite. But whether the amount of yielding is perceptible to observation is a very different question. The effect of such a yielding, if appreciable, would show itself

in the ordinary ocean tides on the earth, diminishing their apparent magnitudes. For the direct solution of this question the British Association, on the motion of Kelvin, appointed a committee to make careful tide measurements. These, together with the valuable tidal observations published by the Indian Government, have been worked up by G. H. Darwin, whose elaborate papers on the tidal stresses in viscous and elastic globes rank among the classical memoirs of the [nineteenth] century. Besides the usual solar and lunar diurnal tides known to every one, there are tides of long periods not generally mentioned in elementary textbooks. These are the fortnightly, the monthly, the semi-annual, and annual tides. The first and third arise respectively from changes in the moon's and sun's declinations, or angular distances N. or S. of the equatorial plane; the second and fourth from changes in their linear distances from the earth. Thus, when the moon's declination is zero there is on the whole higher water at the equator and lower water at the poles than when the declination has any other value. Hence there is a small fortnightly oscillation in the values of the semi-diurnal tides as the moon passes from node to node; and a similar effect with a half year period is produced by the sun's motions in declination. In one revolution again the moon's distance from the earth goes through a complete cycle from apogee to apogee. In perigee the tidal effect, which varies inversely as the cube of the distance, is of course at a maximum. And thus arises the monthly tide; and similarly the annual tide. Of these the fortnightly and monthly tides are much the larger; and it is from them that Darwin draws his conclusions. One advantage in taking these long periodic tides from which to calculate is that, if the earth really does yield, it has time to make an adjustment which can be treated on the ordinary equilibrium theory. In all probability the semi-diurnal tidal stresses are too rapid to produce measurable deformation in the solid earth. This question of *time* in producing strain in viscous or solid matter is really a most important one, and seems to have been quite overlooked in earthquake literature. We shall return to it later. Darwin's method,

then, consists in expressing by means of suitable formulae the fortnightly and monthly tides. In these there enter certain unknown co-efficients, which however have different definite values or relations to each other, according as the earth yields as a whole or is infinitely rigid. By combining the tidal observations of India, Britain, and France, he deduces the most probable values of these co-efficients ; and the final result is that although "there is some evidence of a tidal yielding of the earth's mass, that yielding is certainly small, and that the effective rigidity is at least as great as that of steel".

This conclusion, that the earth as a whole is as rigid as an equal sized globe of steel, seems at first somewhat startling —especially to one who has no very clear notion of what is meant by the term rigidity, but regards it as synonymous with inflexibility. A window pane will rather break than bend; and yet a glass fibre can be used as a thread. The rigidity is the same in both cases, but the inflexibility is much higher in the one case than in the other. The same truth is illustrated in the behaviour of a steel hair-spring and a steel bar. Thus the mere existence of magnificent foldings or rumplings in the hard rocky strata that form the earth's crust can tell us absolutely nothing regarding the rigidity of the material. A sheet of the hardest steel of the same form and size as (say) a stratum of old Red Sandstone would go through very similar transformations under the continued moulding action of the powerful stresses that are known to exist within the earth's substance. To measure rigidity we must know not only the strain produced but the stress which produces it. No purely geological method can lead us to a knowledge of what this stress is; it can be estimated from the strain only when the rigidity is known. Laboratory experiments, however, are of little use in this quest, since we have access to a very limited part of the earth's substance; and even had we access to the deeper regions of our globe the samples finally experimented on would certainly have quite changed their properties when brought from their warm high pressure depths to the light of day.

Darwin has proved, then, that if there exists any tidal

yielding of the earth as a whole it is hardly appreciable, even when searched for by our most refined methods of analysis. Still, although there may be no yielding as a whole, there must be time variations in the tidal stresses at any given point; and these may cause earthquakes if the region chance to be seismically sensitive. No one so far as I know has grouped earthquakes according to lunar or solar declination. Perrey made elaborate comparisons between earthquake frequency and the position of the moon, and obtained apparently definite results. He found that the frequency of earthquakes increased at syzygies, at apogee, and at meridian passage. The last can hardly be accepted as indicating any physical relation, for it is surely impossible for such a short-timed periodic change to have any such pronounced effect. Then, according to Chaplin,[1] the Japan earthquakes have a minimum of frequency at syzygies; and the still more recent discussion by Forel[2] of the Swiss earthquakes throws strong doubt on any such relation. He finds only 53 per cent. of the total number occurring during the syzygy period. The same percentage is brought out for the earthquakes occurring at the meridian passages of the moon. It might be interesting to tabulate earthquakes according to the moon's declination[3]—though it is extremely doubtful if the result obtained would have a value at all worthy of the labour involved. It is possible, however, that this fortnightly oscillation of the direction of maximum tidal stress in the earth's substance may be a determining factor in their frequency.

'Whatever may be the effect of the moon's motion in declination, that of the sun's will of course be much less. Its distance is greater; and its motion in declination smaller. But on the other hand, the period is longer, so that the earth may yield more in proportion to the corresponding variations of stress. One suggestive fact in connexion with these motions in declination is that earthquakes abound in tropical and sub-tropical regions—just where the tidal stresses

[1] *Transactions of Asiatic Society of Japan*, vol. vi.
[2] See *L'Astronomie* (January, 1884).
[3] This I did later—see below, chapter viii.

are greatest, and where also the fortnightly and semi-annual variations most tell. Then there is the solar annual tide, due to the periodic change in the sun's distance. The effect of this would be to cause greater earthquake frequency in the half year from September to March, that is in our winter. In the northern hemisphere there is a very marked winter maximum of frequency; but in the southern hemisphere the maximum seems to come between June and September, while in certain equatorial regions there is no definite annual periodicity at all. We must not, however, conclude hastily that the annual tide has no effect; all we can say is, that there is some other more efficient cause tending to produce earth tremors in the *winter* season, wherever there is a well-marked winter season. Still it is well to consider more in detail what features might be expected to accompany a periodicity determined by the semi-annual and annual tides. As already pointed out, the torrid zone, with immediately contiguous zones to the north and south, would be the seat of most frequent seismic actions. Then, the semi-annual maximum would occur at localities in higher latitudes during the local summer; while the annual maximum would occur about December or January, to allow for the lagging of strain after stress. At equatorial places, the semi-annual effect would really have a quarter-yearly period; while at points lying in the 10th or 12th latitude line, both north and south, this effect would vanish. In many of the statistics there are indications of semi-annual periods—as for example in Mallet's general curve of frequency for the whole northern hemisphere.[1] In searching for such semi-annual periods, we must adopt some means for smoothing off the jaggedness in our curve of frequency and eliminating the annual period. The method which seems to me to be least open to objection is to take overlapping means of the numbers originally obtained.

'Hitherto it must be confessed there has been a good deal of arbitrariness in the treatment of earthquake statistics. As a rule, earthquakes are numbered in monthly groups, which are then combined in seasonal sets. Mallet called

[1] *British Association Reports* (1858).

January, February, March, the winter months—a nomenclature to which strong objection may well be taken—and his example has been generally followed. December, January, February, are certainly a truer seasonal combination, thus making Spring begin on the 1st of March, Summer on the 1st of June, and so on. But this grouping in months is purely arbitrary—since our month is a civil and not a natural division of time. Such a grouping may indicate the existence of long periods, but it affords no ready means of distinguishing between the co-existence of several distinct long periods. To do so, we must devise some method of analysing the complex period shown in these monthly sums. For the northern hemisphere there is a well-marked annual period in earthquake frequency. Is there a Semi-annual period ?—such as might be caused by the changes in the sun's declination.

'To answer this question we must do three distinct operations. We must *prepare* our numbers by some strictly accurate mathematical process so as to magnify (first) the annual effect and (second) the semi-annual effect, smoothing away somewhat the effects of the smaller periods. And then, by direct comparison of these prepared curves, we must separate out the semi-annual period.

'Take for example Professor Milne's catalogue of Japan earthquakes from 1872 to 1880 inclusive, arranged according to months. Form three-monthly means, tabulating under January the mean of December, January, February; under February the mean of January, February, March; under March the mean of February, March, April; and so on through the whole twelve months. The effect of this operation is to magnify the annual and semi-annual periods at the expense of the shorter periods; while the quarter annual period, eighth-annual period, sixteenth-annual period, if there are any, vanish. Beginning again with the original monthly numbers, take the six-monthly overlapping means, and tabulate them as follows: set the mean for the months from January to June halfway between March and April; the mean from February to July halfway between April and May; and so on. By this process we throw out completely semi-

annual, quarter-annual, eighth-annual, &c., periods, and diminish very markedly the effect of all other possible periods.

'Now it can be shown by an application of Fourier's Theorem that from these two curves a third curve may be obtained from which the annual period is completely eliminated. To effect this, the three-monthly means must be reduced by multiplying by the factor ·707. In this way we obtain a prepared semi-annual curve with the annual periodicity involved exactly as it is involved in the annual curve formed from the six-monthly means.'

Applying this method of investigation to selected material available at the time I discussed the annual and semi-annual periodicities of earthquakes in Japan, Europe (Perrey's list as given by Mallet), New Zealand, East Indian Archipelago, Chili, and Grecian Archipelago. By an oversight I inverted the semi-annual curve, so that what was formerly described as the maximum should be the minimum, and the minimum the maximum.

Since 1884, statistics of earthquakes have accumulated at an increasing rate from all parts of the world; and in a paper on the Annual and Semi-annual Seismic Periods,[1] Dr. Davison followed up these early investigations by making use of a much larger body of statistics. He used a slightly modified method of reduction, and deduced not only the times of the maxima and minima, but also compared the amplitudes of the annual and semi-annual periodicities. With the correction of the oversight mentioned above his results corroborate those obtained in 1884.

It is convenient at this stage to consider the general question of the search of periodicities in given statistics. The recognized mathematical method is that furnished by Fourier's analysis of a complex harmonic function into its simple harmonic components. Given, for example, a body of statistics of earthquakes arranged as above according to the month of occurrence, we obtain a set of numbers, twelve (or, better, twenty-four) in all, giving the frequencies in the successive months (or half months) from January to December. If we measure along a horizontal line twelve

[1] *Philosophical Transactions*, vol. clxxxiv, 1893.

equal intervals, representing the successive months, and draw at the middle point of each a vertical line representing to an assumed convenient scale the corresponding frequency, we obtain a graph showing to the eye the march of frequency with time throughout the year. This represents a complex harmonic function of the time. By means of Fourier's theorem we can analyse this function into simple harmonic components, the periods of which are as the reciprocals of the natural numbers, 1, 2, 3, 4, &c. The period of the first component is one year, of the second half a year, of the third four months, of the fourth three months, and so on.

In regard to the value of Harmonic Analysis in such investigations my opinion has considerably altered in recent years. It is a purely mathematical method and leads to results which have in many cases no physical meaning. For example, in the present inquiry our hope is to obtain well-marked annual and semi-annual periods; but the Fourier analysis may give in addition a period of a third of a year with a well-marked amplitude. We might expect that this third harmonic might become less and less significant as we based our investigation on a greater and greater number of events. But this expectation is founded on the hypothesis, generally made unconsciously, that the acting causes which give rise to the first and second harmonics are themselves of a simple harmonic form. Every attempt to analyse a complex harmonic function into simple harmonic components is either a purely mathematical operation enabling us *simply* to build up the original function by putting together the components again, or, if it has any physical significance at all, it is based implicitly on two very doubtful assumptions,—(1) that the acting causes are themselves simple harmonic in their variation, and (2) that this simple harmonic quality is *dynamically reproducible* in the system acted upon. The whole theory of forced vibrations already discussed in chapter v shows how unwarranted this latter assumption may be.

But even were the method of harmonic analysis theoretically unobjectionable, it is extremely doubtful if the

character of earthquake statistics is such as to warrant us in undertaking the labour involved in applying the Fourier analysis. Twelve years ago I went through this labour in connexion with the lunar periodicities discussed below; but I am now convinced that sufficient accuracy is attained by use of the purely arithmetical overlapping summations over suitable intervals. This is essentially the same method as that of the overlapping means used by myself in 1884 and by Davison in 1893.

To illustrate the method which is adopted consistently throughout the present discussion, take the case already referred to, namely, Milne's catalogue of Japan earthquakes from 1872 to 1880 inclusive. This I take because it is historically the first set of statistics to which the arithmetical method for separating out the annual and semi-annual periods was applied.

The monthly frequencies are first arranged in a column, and from these a second column is formed of six-monthly summations, each summation being set exactly half-way down the six-monthly numbers from which it has been formed.

| | Monthly whole year | Six-monthly summations | Monthly half-year | Three-monthly summations |
|---|---|---|---|---|
| Jan. | 29 | | 58 | 189 |
| | | 207 | | |
| Feb. | 36 | | 57 | 166 |
| | | 202 | | |
| Mar. | 38 | | 51 | 166 |
| | | 194 | | |
| Apr. | 26 | | 58 | 178 |
| | | 194 | | |
| May | 32 | | 69 | 201 |
| | | 179 | | |
| June | 33 | | 74 | 201 |
| | | 154 | | |
| July | 29 | | | |
| | | 160 | | |
| Aug. | 21 | | | |
| | | 165 | | |
| Sept. | 13 | | | |
| | | 173 | | |
| Oct. | 32 | | | |
| | | 173 | | |
| Nov. | 37 | | | |
| | | 188 | | |
| Dec. | 41 | | | |
| | | 213 | | |

The third column, headed 'monthly half-year', is formed by adding the frequencies of each pair of numbers six months apart, that is, January and July, February and August, and so on. From these the fourth column of numbers is formed by taking overlapping three-monthly summations. The second and fourth columns give respectively, to a sufficient approximation, the annual and semi-annual periodicities. The annual periodicity shows a marked maximum between December and January, with a minimum between June and July. The difference between the maximum and minimum divided by the total number of earthquakes, namely $(213-154)/367$ or 0·161, gives what may be called the uncorrected relative amplitude. Similarly for the semi-annual periodicity we find $35/367 = 0{\cdot}095$; and the maximum falls between May and June and also between November and December.

This method of preparing the numbers in order to be able to apply to the semi-annual periodicity the method already used for deducing the annual periodicity is the modification introduced by Davison. It is simpler of application and not less accurate than the method originally used by me.

We must now consider how to correct the relative amplitude so as to obtain what may be regarded as the true value of the amplitude in the periodic variation under discussion. Let us suppose that the function is of the form

$$f = F + A\cos(t+a) + B\cos(2t+b) + \text{\&c.}$$

where $t$ is the time expressed in the unit of which $2\pi$ or 6·2832 represents the complete period under discussion. What we use in the statistical discussion is not $f$ but the summation or integral of $f$ through a limited fraction of the period, in the present case, half a month. Let $2\pi/n$ represent this limited interval of time. Then the tabulated frequencies may be represented by the expression

$$\begin{aligned} f_1 = \int_{t-\pi/n}^{t+\pi/n} f\,dt &= \frac{2\pi}{n}F + 2A\sin\frac{\pi}{n}\cos(t+a) \\ &\qquad + 2B\,\tfrac{1}{2}\sin\frac{2\pi}{n}\cos(2t+b) \\ &= \frac{2\pi}{n}F\left\{1 + \frac{A}{F}\frac{\sin\pi/n}{\pi/n}\cos(t+a)\right. \\ &\qquad \left. + \frac{B}{F}\frac{\sin 2\pi/n}{2\pi/n}\cos(2t+b) + \ldots\right\}. \end{aligned}$$

This quantity we then treat by overlapping summations, which correspond to integrations over half the period. We find

$$f_2 = \int_{t-\pi/2}^{t+\pi/2} f_1 dt = \frac{2\pi F}{n}\left\{\pi + \frac{A}{F}\frac{\sin\pi/n}{\pi/n}\frac{2\sin\pi/2}{1}\cos(t+a) + \frac{B}{F}\frac{\sin 2\pi/n}{2\pi/n}\frac{2\sin\pi}{2}\cos(2t+b) + \ldots\right\}$$

$$= \frac{2\pi^2 F}{n}\left\{1 + \frac{A}{F}\frac{\sin\pi/n}{\pi/n}\frac{2}{\pi}\cos(t+a) + \ldots\right\}$$

the term in $B$ vanishing because $sin\ \pi = 0$.

Thus the uncorrected relative amplitude is equal to the true relative amplitude $A/F$ multiplied by the factor

$$\frac{2n}{\pi^2}\sin\frac{\pi}{n}$$

where $1/n$ is the fraction of the whole period in terms of which the frequencies are tabulated. In the case already discussed, $n$ is 12 for the annual, 6 for the semi-annual. When the frequencies are tabulated by half-months, then the corresponding numbers will be 24 and 12, with 6 for the quarter-annual. When we come to discuss the lunar monthly periods, the frequencies are tabulated by days, and $n$ is 28 or 30 according to the kind of month, and 14 or 15 for the half-monthly period corresponding. Again, in the solar daily and half-daily periods, $n$ is 24 and 12 respectively; while in the lunar daily and half-daily statistics we take $n$ equal to 25 and 12·5 respectively. It is convenient to calculate the factor for these various cases, as in the following table:

| $n =$ | 3 | 6 | 12 | 24 | 12.5 | 25 | $\infty$ |
|---|---|---|---|---|---|---|---|
| $\frac{2n}{\pi^2}\sin\frac{\pi}{n} =$ | ·527 | ·608 | ·629 | ·634 | ·630 | ·635 | ·637 |

Hence to pass from the uncorrected amplitude to the true relative amplitude we divide by one or other of these fractions, according to the method of grouping adopted.

For values of $n$ above 6 the factor does not change much in value; and in most cases when all that is aimed at is a comparison of the amplitudes of the harmonic components whose periods are as 1 to 1/2, the uncorrected amplitudes

are sufficiently accurate. There is, however, another consideration which compels us to introduce these factors.

In 1897, Professor Schuster communicated a paper to the Royal Society of London on Lunar and Solar Periodicities of Earthquakes, which was to a large extent a critical examination of the results indicated in my somewhat earlier paper on the same subject. These will be discussed later. It is important, however, to refer at this place to Schuster's critique, which furnishes a criterion as to the reality of an apparent periodicity in such statistics as we are dealing with.

The question is one of probabilities; and, developing a result given by Rayleigh in 1880, Schuster draws certain conclusions which may be described in the following terms. Suppose that we have $n$ disconnected events occurring at random within a given interval of time, and that we consider the probability of the frequency of these events being expressed harmonically by a Fourier series in which the periods are submultiples of the interval of time; then it is shown that the probability of any amplitude lying between the limits $r$ and $r+dr$ is

$$\tfrac{1}{2}nr\,dr\,e^{-nr^2/4};$$

that the expectancy for the value of the amplitude is $\sqrt{(\pi/n)}$; and that the probability of the amplitude exceeding any given value $p$ is $exp\,(-np^2/4)$.

To appreciate the bearing of this application of the mathematical laws of probability, consider the annual periodicities of three cases worked out in my first paper of 1886, namely, Perrey's list of European historic earthquakes as given by Mallet, the list of Japanese shocks reproduced above, and the shocks experienced in the Grecian archipelago. The number of independent shocks in these cases were, respectively, 1961, 367, and 3587. The corresponding values of Schuster's 'expectancy' are 0·04, 0·0925, 0·0296. The uncorrected relative amplitudes are 0·14, 0·14, and 0·102, giving, when divided by 0·63, the corrected values 0·22, 0·22, and 0·16. In the first and third cases the amplitudes are distinctly greater than the calculated expectancies; but although the amplitude is greater than the

expectancy in the case of the Japan statistics, it is not markedly so. The probability that the amplitude should exceed 0·2, the events being assumed to be disconnected, comes out in the respective cases, $1/(33 \times 10^7)$, $1/39$, $1/(38 \times 10^{14})$. Thus, even in the case in which the relative amplitude is a little more than twice the expectancy for random disconnected events, the probability that these will give a value greater than the amplitude obtained is only 1 in 40.

The following table gives the probabilities that the amplitudes should exceed successive multiples of the expectancy $E$, and shows how rapidly the probability diminishes as the multiples increase.

| Amplitude = | $E$ | $2E$ | $3E$ | $4E$ | $5E$ | $6E$ | $8E$ | $10E$ |
|---|---|---|---|---|---|---|---|---|
| Probability = | 0·456 | ·0432 | $\cdot 0_3 85$ | $\cdot 0_5 35$ | $\cdot 0_8 3$ | $\cdot 0_{12} 52$ | $\cdot 0_{21} 15$ | $\cdot 0_{34} 78$ |

The suffix after each zero indicates how many zeros there are after the decimal point. When the amplitude reaches four times the expectancy the probability is so small that we are justified in regarding the existence of such an amplitude as evidence of a real periodicity.

We now proceed to consider the evidence furnished by various sets of statistics of earthquake frequency.

I give first a table containing the results deduced from the six sets of earthquake statistics which I discussed in 1884. The successive columns of the table give the name of the region, the interval of time over which the statistics extend, the number of shocks catalogued, the month in which the annual maximum occurs, the (earlier) month in which the semi-annual maximum occurs, the corrected relative amplitudes, and the 'expectancy' according to Schuster's theory.

| Region | Time Interval | No. of Shocks | Month of Maximum | | Amplitude | | Expectancy $\sqrt{(\pi/n)}$ |
|---|---|---|---|---|---|---|---|
| | | | Annual | Semi-ann. | Annual | Semi-ann. | |
| Japan . . | 1872–80 | 367 | XII–I | III | 0·256 | 0·157 | 0·093 |
| Europe . | 306–1843 | 1961 | XII | I | 0·22 | 0·12 | 0·04 |
| N. Zealand | 1869–79 | 585 | VIII–IX | II | 0·203 | 0·161 | 0·073 |
| E. Indian Archip. | 1873–81 | 515 | VIII, X, or XII ? | III | 0·071 ? | 0·099 | 0·078 |
| Chili . . | 1873–81 | 212 | VII | VI | 0·48 | 0·17 | 0·122 |
| Grecian Archip. | 1859–81 | 3578 | XII–I | III | 0·164 | 0·268 | 0·03 |

These all, with one important exception, indicate a marked annual periodicity, with amplitudes well above the expectancy value for disconnected events. The time of maximum is, in every case, during the winter season. The one exception is the case of the East Indian Archipelago, for which the earthquakes show no distinct tendency to a maximum, and for which there is no marked winter season.

Before discussing the significance of these results let us consider the corresponding results obtained by Davison from the much larger body of statistics to which he had access ten years later.

It seems to me that in many cases the statistics utilized by Davison are too meagre for the purpose proposed; and it is doubtful if, even in the best sets of statistics, we should lay any great stress on numerical details. Some of the catalogues at our disposal cover centuries, and yet the number of earthquakes recorded does not exceed a few thousands. These are, of course, large shocks which receive mention in ordinary historic records. Very recent catalogues, again, are in default in another direction. They extend over too small an interval of time. In other cases the interval of time may be sufficient, but the total number of shocks is so small as to give an average of less than 20 a year. It is evident that in such cases, unless the interval is distinctly long, extending over a century or two, the statistics are really not sufficient to serve as a basis for calculations in periodicities.

Of the 62 different sets of records made use of by Davison, only 23 may be regarded as satisfactory, and about 7 others barely admissible. It is impossible, for the reasons given, to place any confidence in results deduced from the remaining 32 cases.

In the following table, constructed on the same principle as that just given, I have arranged the sets of records which seem worthy of consideration, leaving out those sets already discussed by me and rediscussed by Davison.

Because of the comparatively small annual frequency the last five are of little account.

EARTHQUAKE STATISTICS (DAVISON)

| Region | Limiting Dates | No. of Shocks | Maximum Month | | Amplitude | | Expect. $\sqrt{(\pi / n)}$ |
|---|---|---|---|---|---|---|---|
| | | | Annual | Semi-ann. | Annual | Semi-ann. | |
| N. Hemisphere . | 223–1850 | 5879 | XII | II | 0·11 | 0·07 | 0·023 |
| N. Hemisphere . | 1865–84 | 8133 | XII | IV | 0·29 | 0·11 | 0·02 |
| Europe . . . . | 1865–84 | 5499 | XII | IV | 0·35 | 0·11 | 0·024 |
| Austria . . . . | 1865–84 | 461 | I | V–VI | 0·37 | 0·22 | 0·083 |
| Switzerland and Tyrol . . . . | 1865–83 | 524 | I | VI | 0·56 | 0·37 | 0·077 |
| Italy . . . . | 1865–83 | 2350 | IX, XII | IV | 0·14 | 0·14 | 0·037 |
| Italy, excluding Sicily and Vesuvius . . . . | 1865–83 | 1513 | IX, XI | IV | 0·21 | 0·17 | 0·046 |
| Vesuvius District | 1865–83 | 513 | XII | VI | 0·25 | 0·25 | 0·078 |
| Italy Old Tromometre | 1872–87 | 61732 | XII | V–VI | 0·49 | 0·08 | 0·0071 |
| Old „ | 1876–87 | 38546 | XII | V | 0·46 | 0·04 | 0·0093 |
| Normal „ | 1876–87 | 38546 | XII | V | 0·49 | 0·03 | 0·0093 |
| S. E. Europe . . | 1859–87 | 3470 | XII | III | 0·21 | 0·25 | 0·03 |
| Balkan, &c. . . | 1865–84 | 624 | XII | II | 0·27 | 0·37 | 0·071 |
| Zante . . . . | 1825–63 | 1326 | VIII | VI | 0·10 | 0·19 | 0·049 |
| Japan . . . . | 1885–89 | 2997 | X | III | 0·08 | 0·07 | 0·032 |
| Japan . . . . | 1876–81, 1883–91 | 1104 | II | V | 0·19 | 0·21 | 0·053 |
| Japan . . . . | 1878–81 | 246 | XII | I | 0·46 | 0·19 | 0·113 |
| Malay Archip. . | 1865–84 | 598 | V | I | 0·19 | 0·25 | 0·072 |
| S. Hemisphere . | 1865–84 | 751 | VII | I, III | 0·37 | 0·06 | 0·065 |
| New Zealand . . | 1868–90 | 641 | III, V | II | 0·05 | 0·13 | 0·070 |
| Chili . . . . . | 1865–83 ? | 316 | VII, XII | IV | 0·27 | 0·11 | 0·100 |
| Hungary, &c. . . | 1865–84 | 384 | XII | VI | 0·31 | 0·30 | 0·090 |
| Asia . . . . . | 1865–84 | 458 | II | II | 0·33 | 0·14 | 0·083 |
| N. America . . | 1865–84 | 552 | XI | IV | 0·35 | 0·14 | 0·075 |
| California . . . | 1850–86 | 949 | X | IV | 0·30 | 0·16 | 0·058 |
| Peru, Bolivia . . | 1865–84 | 350 | VII | I | 0·48 | 0·24 | 0·095 |

An examination of the larger table will be found to corroborate in a remarkable way the conclusions derived from the much smaller body of statistics used in 1884. These early statistics were to some extent selected with a view of testing the existence of a 'seasonal' periodicity which might be connected with meteorological rather than with gravitational tidal effects.

Thus we see that, with three exceptions, namely, Zante, the Malay Archipelago, and (what is practically the same) the East Indian Archipelago, the maximum annual frequency occurs in a winter season month. This month is December in the great majority of the northern hemisphere regions ;

while in the southern hemisphere the maximum frequency falls in the autumn and winter seasons of that hemisphere.

The Malay Archipelago statistics given by Fuchs, on which Davison based his discussion, show a somewhat small maximum in May, whereas the East Indian statistics which I used gave no clear annual maximum. In this latter case the semi-annual maximum is not much greater than the expectancy, whereas Fuchs's statistics give a semi-annual amplitude fully three times the expectancy. Fuchs's statistics cannot be very complete, for he gives 598 shocks in nineteen years, while Bergsma, who was my authority, gave 515 shocks in only eight years.

To bring out the broad distinction of the annual variation of earthquake frequency in the two hemispheres I reproduce the statistics as given by Fuchs and arranged by Davison. They are grouped in half-month intervals, each number in the lower line referring to the second half of the month named.

| Hemisphere | J. | F. | M. | A. | M. | J. | J. | A. | S. | O. | N. | D. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Northern . | 342 | 344 | 403 | 286 | 345 | 242 | 269 | 237 | 252 | 343 | 487 | 437 |
| 1865–84 . | 409 | 338 | 405 | 269 | 285 | 256 | 321 | 244 | 318 | 422 | 505 | 374 |
| Southern . | 35 | 24 | 23 | 23 | 22 | 40 | 33 | 41 | 27 | 48 | 31 | 18 |
| 1865–84 . | 25 | 23 | 35 | 23 | 20 | 45 | 43 | 47 | 23 | 53 | 33 | 16 |

The numbers show at a glance that the frequency in the northern hemisphere is fully ten times that in the southern—a fact which is immediately attributable to the greater diversity of land and sea in the northern hemisphere.

In the diagram I reproduce Davison's graphs of the annual and semi-annual periodicities for the two hemispheres. The full line curves refer to the annual periodicity; the dotted lines to the semi-annual.

The seasonal significance of the annual periodicity is undoubted; in each case the maximum frequency falls in the winter season. The greater range in the case of the southern hemisphere is to be attributed to the smaller number of earthquakes involved. But it is otherwise with the semi-annual graph. There, in spite of the much smaller number of items in the statistics, the semi-annual periodicity is distinctly less marked in the southern hemisphere

than in the northern. When, however, we consider the

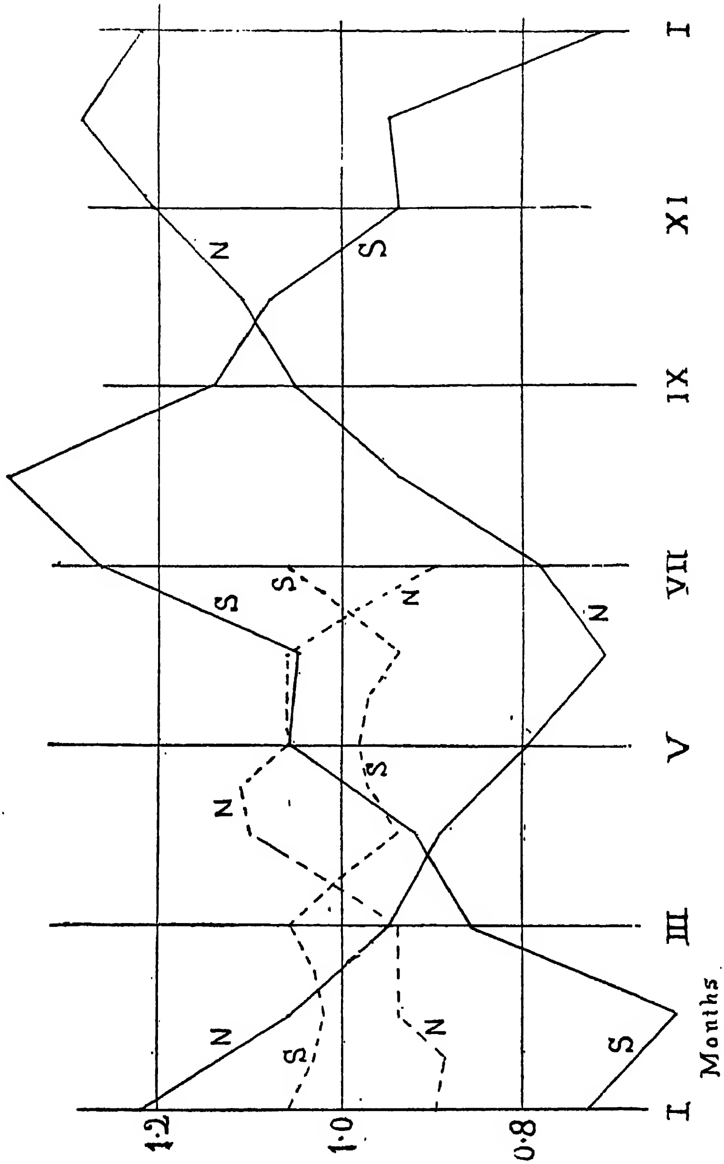



FIG. 27.

'Expectancy' (see the table on p. 116) we cannot regard the

apparent semi-annual periodicity as established. In 1884 I wrote that 'the present investigation certainly indicates a semi-annual period, but an accumulation of observations is needed to establish it as more than conjectural'. Writing now, twenty-two years later, I confess that the evidence is, as a whole, no stronger than it was. There is too great a variety in the times of the maxima, and the amplitude in many cases is also too small when due regard is paid to the number of observations which form the basis of the investigation.

At the same time a comparison of the graphs for the two hemispheres suggests the possibility of a semi-annual periodicity which must be due to a cause less effective in the southern than in the northern hemisphere.

In 1884 I was led by a process of exhaustion to the conclusion that the only two possible causes of the annual and semi-annual periodicities were accumulations of snow over continent-areas and slow variations of barometric pressure, to which might be added rainfall, denudation, and deposition. The summary of the argument was as follows:

'The cause of earthquakes is probably to be referred to the earth's heterogeneity of structure or to the inequality of stress due to irregularities of its surface. Rupturing or yielding is not determined by the amount of stress only; it depends in great measure upon how the stress is applied. For rupture to take place the stress must be different in different directions; and the difference between the greatest and least stresses is an important datum in estimating the tendency to break. So far as can be judged, the only periodic stresses that exist of period long enough to tell upon the earth's substance are the fortnightly, monthly, semi-annual, and annual tides, the annual variation of snowfall, and the steady annual and perhaps semi-annual oscillation of barometric pressure over the earth's surface. Inasmuch as the earthquake frequency reaches its maximum in winter wherever there is a marked winter season, we must pass from the annual tidal stress due to the sun as of little account. We seem, however, to find in the accumulations of winter snow, and in the long period oscillations of the atmospheric pressure, two possible determining factors in earthquake frequency.'

The views advanced by Dr. Davison differ from this only

in one respect. He has more regard to the annual change of pressure over the seismic district itself; whereas I take into account rather the whole manner in which the pressure varies across the seismic region.

On referring to any meteorological atlas and comparing the distribution of barometric pressure for the two months January and July, we see that in January the areas of high pressure are over the continents in the northern hemisphere, and over the seas in the southern hemisphere. In July, the opposite conditions hold, the low pressure being over the continents in the northern hemisphere and over the seas in the southern hemisphere. That is to say, in the winter season pressure increases over the land surfaces in all but tropical regions; while in the summer season the pressure is greater over the ocean. This see-saw of condition is particularly well-marked in the northern hemisphere, which is characterized by preponderance of continental areas. But not only does the pressure increase over the land areas in the seasonal winter, but, as is obvious at a glance at the isobaric charts, the isobars are more crowded together when the land supports the higher pressure than when (in the summer season) the high pressure is over the seas. That is to say, the gradient is steeper in winter as we pass out from the land towards the sea than in summer as we pass from the sea to the land. It is with this condition that I associate the winter maximum of earthquakes, rather than with the purely local state of the barometer. It is a condition which tends to increase the slight instability of the isostatic state which is always more or less present on the margins of continents. It has been pointed out both by Davison and Omori that the yielding of the ocean waters to changes of barometric pressure prevents any corresponding changes of pressure on the ocean bottom; but that fact no more affects the distribution over the land and its consequences than does the much more important daily tide as it sweeps over the seas. This barometric gradient is one of the factors which affects the isostatic stability, tending, like rainfall, snowfall, denudation of material from the heights, and its accumulation along shores, to produce an increased

instability which is followed by yielding and by the assumption of a new and temporarily more stable configuration.

If we adopt the generally accepted view that earthquakes are slips along faults, we see at once that a vertical load, varying in amount as we pass along the surface, is just the kind of thing to increase any tendency to slip which may be present. The mere increase of load over a definite area does not seem to me to involve of necessity any greater tendency to slip in the region immediately beneath. As a matter of fact all barometric changes over certain regions are accompanied by gradients more or less steep in varying directions; but during the winter months, over the northern hemisphere especially, there is a prolonged condition of average barometric distribution giving a pronounced gradient over the littoral countries. This seems to me to be the most plausible way of looking at any relation between annual barometric change and seismic frequency.

I have reproduced the argument in some detail because, apparently, it was not quite understood as presented in my early paper. That paper also contained a hint at a possible cause for the semi-annual periodicity of which there seemed to be some evidence. This was found in the fact that the barometric gradient, that is, the variation of pressure along the surface of the earth, really attains a second but much smaller maximum oppositely directed during summer. For at that season there is minimum pressure over the land and maximum over the seas. The evidence for the semi-annual periodicity seems to me to be somewhat precarious. There is considerable diversity among the times of the maxima in the different sets of records. If all were like the two of which I have given the graphs the evidence would be greatly strengthened. It is at least curious that the southern hemisphere shocks should show a semi-annual periodicity so much smaller in amplitude than that given by the more numerous statistics of the northern hemisphere. Also very suggestive is the fact that the southern semi-annual maximum occurs at the same time as the northern semi-annual minimum, and vice versa.

This points to a seasonal effect, just as in the case of the annual periodicities; and the comparative smallness of the amplitude of the southern semi-annual periodicity may be connected with the fact that in the southern hemisphere there is less land and a closer approximation to the uniform conditions of all ocean.

The problem of the annual periodicity in Japan has been recently studied with considerable elaboration by Omori.[1] One of his general conclusions is that Japan may be divided into two districts, in one of which the annual maximum occurs in winter and in the other in summer. Roughly stated the conclusion is that the NE. end of the main island of Japan and the E. of Yezo are characterized by a summer maximum; while the west and middle parts (with the exception of a few isolated portions of limited extent) are characterized by a winter maximum. A critical examination of the districts classified by Omori shows that the majority of these are insufficient by themselves to establish any law of frequency. When dealing statistically with a limited district we must have records extending over a long period of years to make up for the usually meagre number of shocks recorded per year. There is, however, a more fundamental source of uncertainty in the separate lists prepared by Omori. In investigations of this kind we should have regard rather to the source of the shock than to the locality where it is felt. For small local shocks there is of course no difficulty; but for shocks felt over a fairly wide area it is otherwise. Thus, in the Tokyo list which I reproduce here, the Mino-Owari disaster of October, 1891, is represented by the number 45, whereas in the immediately preceding month there are only 4 shocks recorded. In taking his monthly means Omori excluded this number 45 on the ground that these were aftershocks of a strong earthquake. It is very doubtful if all the 45 were so; but admitting that they were, what is to be said of the 12 and 15 in the immediately succeeding months? Some of these must have been as truly aftershocks of the great earthquake of October as the majority of those which were

[1] *Publications of the Earthquake Investigation Commission*, No. 8, 1902.

felt in Tokyo in October. The same uncertainty applies not only to those particular cases (March, 1894, June, 1896, and August, 1897) which have been excluded in the taking of the monthly means, but also to other cases which have not been excluded, namely, January, 1896, with its 42 shocks, July and August, 1896, with their 22 and 18 shocks respectively, some of which must have belonged to the same after-shock system as most of the excluded 51 of the preceding month, and so on. It is sufficient to mention these cases to show how difficult it is to segregate shocks according to limited districts. It seems to me that there is a good deal of arbitrariness in the manner in which Omori has excluded certain numbers and included others.

Taking the numbers as they stand, however, let us treat them so as to deduce the annual and semi-annual periods.

Monthly Frequencies at Tokyo (Omori). 1876–99

| Year | Month | | | | | | | | | | | | Mean |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | I | II | III | IV | V | VI | VII | VIII | IX | X | XI | XII | |
| 1876 | 3 | 4 | 6 | 11 | 5 | 3 | 3 | 5 | 3 | 3 | 4 | 6 | 56 |
| 1877 | 4 | 5 | 6 | 5 | 8 | 9 | 6 | 4 | 1 | 8 | 6 | 9 | 71 |
| 1878 | 3 | 8 | 7 | 2 | 5 | 4 | 4 | 1 | 2 | 4 | 6 | 4 | 50 |
| 1879 | 6 | 7 | 14 | 0 | 9 | 4 | 3 | 4 | 1 | 7 | 6 | 9 | 70 |
| 1880 | 9 | 9 | 6 | 6 | 2 | 9 | 8 | 4 | 1 | 3 | 10 | 10 | 77 |
| 1881 | 13 | 8 | 8 | 8 | 4 | 3 | 3 | 3 | 2 | 3 | 3 | 8 | 66 |
| 1882 | 4 | 7 | 15 | 6 | 3 | 2 | 2 | 1 | 1 | 4 | 1 | 0 | 46 |
| 1883 | 6 | 0 | 3 | 3 | 6 | 2 | 3 | 1 | 0 | 1 | 3 | 4 | 32 |
| 1884 | 5 | 2 | 8 | 2 | 9 | 4 | 1 | 4 | 2 | 8 | 8 | 15 | 68 |
| 1885 | 7 | 9 | 8 | 4 | 3 | 6 | 0 | 3 | 8 | 10 | 3 | 7 | 68 |
| 1886 | 3 | 3 | 3 | 2 | 8 | 4 | 2 | 8 | 7 | 4 | 2 | 8 | 54 |
| 1887 | 10 | 4 | 3 | 8 | 13 | 5 | 6 | 2 | 10 | 0 | 5 | 14 | 80 |
| 1888 | 4 | 15 | 7 | 7 | 11 | 9 | 9 | 7 | 11 | 4 | 13 | 4 | 101 |
| 1889 | 5 | 16 | 11 | 18 | 13 | 7 | 5 | 8 | 7 | 8 | 9 | 6 | 113 |
| 1890 | 5 | 5 | 6 | 15 | 14 | 5 | 12 | 7 | 4 | 8 | 10 | 2 | 93 |
| 1891 | 1 | 4 | 6 | 7 | 10 | 7 | 8 | 4 | 4 | *45 | 12 | 15 | 123 |
| 1892 | 9 | 11 | 3 | 7 | 7 | 9 | 14 | 2 | 7 | 11 | 4 | 8 | 92 |
| 1893 | 5 | 4 | 5 | 7 | 10 | 10 | 4 | 3 | 6 | 3 | 3 | 1 | 61 |
| 1894 | 7 | 8 | *23 | 11 | 9 | 9 | 6 | 3 | 8 | 4 | 8 | 5 | 101 |
| 1895 | 20 | 9 | 8 | 17 | 11 | 12 | 11 | 5 | 10 | 17 | 6 | 3 | 129 |
| 1896 | 42 | 17 | 15 | 21 | 10 | *51 | 22 | 18 | 7 | 5 | 7 | 10 | 225 |
| 1897 | 11 | 18 | 11 | 8 | 17 | 6 | 11 | *32 | 5 | 8 | 15 | 22 | 164 |
| 1898 | 4 | 8 | 16 | 17 | 16 | 13 | 16 | 13 | 15 | 5 | 11 | 10 | 144 |
| 1899 | 7 | 11 | 17 | 13 | 6 | 9 | 8 | 16 | 8 | 8 | 13 | 8 | 124 |
| sum | 193 | 192 | 215 | 205 | 209 | 202 | 167 | 158 | 130 | 181 | 168 | 188 | 2208 |
| 1876–95 | 129 | 138 | 156 | 146 | 160 | 123 | 110 | 79 | 95 | 155 | 122 | 138 | 1551 |
| 1896–99 | 64 | 54 | 59 | 59 | 49 | 79 | 57 | 79 | 35 | 26 | 46 | 50 | 657 |

The numbers marked with an asterisk are those which Omori has excluded in taking the monthly means. Instead of utilizing his mode of treatment, which is somewhat crude, though possibly no cruder than the material on which he builds, I propose to treat the numbers by the method already explained.

A glance at Omori's table shows that during the last four years the frequency greatly increased. It seemed important in connexion with the criticisms stated above to inquire as to how far a short interval of fairly high seismicity agreed with a longer interval of distinctly less seismicity. The interval from 1876 to 1895, and the interval from 1896 to 1899, have therefore been treated separately. I have also treated the sums of the whole, omitting the monthly numbers marked with an asterisk. This should of course agree with Omori's conclusions based on the monthly means.

A little consideration will show that whereas the semi-annual sums refer to the middle of each month, the annual sums refer to the point of separation of two contiguous months. This necessarily throws the maximum and minimum points of the annual periodicity half a month later than the times of occurrence of the maximum and minimum points of the semi-annual periodicity; but this is of no practical account since it is grotesque to imagine that statistics of the kind can give results correct to half a month. The numbers obtained by this treatment are shown in the two following tables, in each of which there are four columns corresponding to the total sums of Omori's columns for the whole period, the same sums diminished by the numbers marked with an asterisk, the sums for the interval 1876 to 1885, and the sums for the interval 1895 to 1899. These I shall distinguish by the symbols $S$, $S'$, $S$ (20) $S$ (4), for the six-monthly summations giving the annual period, and $T$, $T'$, $T$ (20), $T$ (4), for the three-monthly summations corresponding to the semi-annual periods.

At the foot of each table are given the corrected relative amplitude, and Schuster's 'Expectancy'.

ANNUAL PERIODICITY (SIX-MONTHLY SUMMATIONS)

| Month | $S$ | $S'$ | $S$(20) | $S$(4) |
|---|---|---|---|---|
| I–II | 1161 | 1138 | 829 | 332 |
| II–III | 1202 | 1179 | 867 | 335 |
| III–IV | 1216 | 1142 | 852 | 364 |
| IV–V | 1190 | 1116 | 835 | 357 |
| V–VI | 1156 | 1050 | 775 | 382 |
| VI–VII | 1071 | 988 | 714 | 358 |
| VII–VIII | 1047 | 919 | 723 | 325 |
| VIII–IX | 1006 | 878 | 685 | 322 |
| IX–X | 992 | 915 | 700 | 293 |
| X–XI | 1018 | 941 | 717 | 300 |
| XI–XII | 1052 | 1007 | 777 | 275 |
| XII–I | 1137 | 1069 | 838 | 299 |
| Amplitude | 0·162 | 0·234 | 0·186 | 0·258 |
| Expectancy | 0·038 | 0·039 | 0·045 | 0·069 |

SEMI-ANNUAL PERIODICITY (THREE-MONTHLY SUMMATIONS)

| Month | $T$ | $T'$ | $T$(20) | $T$(4) |
|---|---|---|---|---|
| I or VII | 1100 | 1017 | 784 | 383 |
| II or VIII | 1055 | 1000 | 718 | 348 |
| III or IX | 1081 | 981 | 768 | 312 |
| IV or X | 1108 | 1040 | 834 | 274 |
| V or XI | 1153 | 1057 | 844 | 309 |
| VI or XII | 1127 | 1076 | 784 | 345 |
| Amplitude | 0·074 | 0·075 | 0·053 | 0·230 |
| Expectancy | 0·038 | 0·039 | 0·045 | 0·069 |

The principal facts to be noted are these :

1. The last four years give a maximum from two to three months later than the other groupings ; they give an amplitude distinctly greater than is given by the twenty years' grouping ; and the semi-annual periodicity is considerably increased relatively to the annual periodicity as compared with what holds in the other groupings. This shows merely that the period is too short to base any conclusions on. And if this is the case for the comparatively great frequency in the Tokyo district, it will certainly also be the case for districts of less seismicity. We cannot expect to get good results from meagre statistics. These always give more irregular means, and greater relative amplitudes.

2. Comparing the first two groupings, we see that the exclusion of the marked numbers in Omori's list greatly affects the annual periodicity in amplitude, and brings the maximum frequency time about a month earlier. On the other hand the semi-annual periodicity is hardly affected either in amplitude or in time of maximum. The arbitrary exclusion of all the recorded shocks in particular months on the ground that the majority are aftershocks of a large earthquake in a neighbouring district does not improve the numbers as a body of statistics.

3. The whole twenty-four years of records may be roughly divided into two halves, of which the first is characterized by a much lower seismicity than the second. Thus from 1876 to 1887 the records number 738, whereas from 1888 to 1899 they run up to 1470, just about double. All were observed instrumentally; and it is stated in the introduction that the majority of the seismographs used were of the Gray-Milne type. This form of seismograph was not in existence as early as 1876, and was not in general use till about 1884. There seems to me to be some little uncertainty as to the meaning of this great increase of seismicity since 1887. Not one single year before that date shows so great a frequency as 90, and only one year since that date shows a smaller, while all the others are marked by a distinctly greater frequency. The introduction of more delicate forms of seismograph would of course increase the apparent frequency.

I have considered with some care the Tokyo list of shocks, which is more complete than any of the others, my aim being to come to some understanding as to the certainty with which we may draw broad conclusions from records of shocks felt but not necessarily originating in limited regions. It is very doubtful if we have yet accumulated sufficient material to justify any attempts to establish various periodicities in the seismic frequency of limited regions. Omori himself leaves out of account the returns from certain stations on the ground that there are too few shocks. It seems to me that he might quite reasonably have drawn the line so as to exclude a number of other stations, which have a small

monthly average and a small number of years of observations. My own feeling is that the minimum allowable for statistical work of this kind is an average of three shocks per month over an interval of twelve years. Of the fifteen stations of Omori's *A* group only four stand this test, namely, Tokyo, Nagoya, Gifu, and Kumamoto. Gifu and Nagoya became seismic centres of importance only after the great disaster of October, 1891; and the majority of the thousands of shocks recorded in the eight succeeding years occurred within the first five, being of the nature of aftershocks of the great earthquake. In taking the monthly means, however, Omori excludes the shocks of 1891, 1892, and 1894, so that practically he is dealing with 520 shocks in eight years in the case of Nagoya, and 898 shocks in the same time in the case of Gifu. The Kumamoto list contains 1578 shocks in 10·5 years. The usual substitution of the mean values in certain months instead of the recorded number reduces this to 746 shocks in ten years. Treating this in the same way I find the following results:

| | Annual | Semi-annual |
|---|---|---|
| Maximum month | III–IV | IV and X |
| Amplitude | 0·32 | ·079 |
| Expectancy | ·065 | ·065 |

Comparing this with *S′* for Tokyo we see that Kumamoto and Tokyo give very similar results.

Out of the eleven stations of Omori's *B* group only four can be admitted as satisfying the condition laid down above, namely, Nemuro, Miyako, Ishinomaki, and Fukushima.

| | | | | | | |
|---|---|---|---|---|---|---|
| Nemuro | 1343 | shocks | in | 15 | years | (5 years instrumental) |
| Miyako | 704 | ,, | ,, | 17 | ,, | |
| Ishinomaki | 1034 | ,, | ,, | 14 | ,, | |
| Fukushima | 857 | ,, | ,, | 11 | ,, | |
| Utsunomiya | 492 | ,, | ,, | 9 | ,, | |

the last being just on the margin. Unfortunately even in these cases the instrumental observations are for a very limited number of years. If we take into account only the years of instrumental observations, we find that these number 4 in the case of Miyako, nearly 5 for Fukushima, 6 for Utsunomiya, 9 for Ishinomaki, and 12 for Nemuro.

Treating the Nemuro shocks by the method of overlapping sums I find as follows :

| | Annual | Semi-annual |
|---|---|---|
| Maximum month | VII–VIII | V and XI |
| Amplitude | 0·137 | 0·107 |
| Expectancy | ·048 | ·048 |

The true interpretation of this result is that there is no clear evidence of an annual periodicity at all. The amplitude is comparatively small.

The cumulative evidence from the statistics of these five stations does, however, point towards the conclusion reached by Omori, that along the north-east coast of Japan the frequency tends to a maximum in the warmer months, while in other parts of Japan more frequently visited by earthquakes the maximum tends towards the early months of the year. We need, however, a larger body of statistics to make this conclusion sure, and for this we must wait for a sufficient lapse of time.

Omori points out that the stations of his *B* group are mostly shaken by shocks which have their origins under the ocean, whereas the stations of group *A* are more subject to earthquakes having their sources below the land. If—and Omori adopts this hypothesis also—we are to explain the winter or spring maximum frequency by the annual barometric changes, then it seems hopeless to make this also the cause of the indicated summer or autumn maximum in the case of the stations forming the *B* group. Both Davison and Omori seem, however, to look entirely to the statical pressure over the district considered at the time of maximum frequency. As already pointed out, we should consider the amount of the barometric gradient across the district and the changes in that gradient as the year goes on. It must be because of the change in distribution that the frequency is affected. With no change there would be no changing influence upon the seismically sensitive region ; and this we must have if there is to be a corresponding periodicity in the seismic frequency. The change in distribution may be of two kinds. There is first the space change, or gradient,

producing varying loads as we pass across any region of the earth's surface, and then there is the time change, which means a changing gradient with accompanying changes of differential load. These at least are possible dynamical causes, not only of an annual, but also of a semi-annual, periodicity.

There are, however, other meteorological phenomena which might with reason be expected to influence the seismic frequency. There is the accumulation of snow over land areas in winter and spring. Also the rate of denudation of the uplands and the forming of new deposits along the shores will be affected by the periodicity of the rainfall and the melting of the snows; and this, by altering the loading, will affect the isostasy of the land masses and influence the seismic frequency. It is hardly possible to credit the solar radiation with any direct influence; for, as we learnt long ago from the measurements by Forbes of underground temperature, solar radiation penetrates a very short distance into the earth's crust.

# CHAPTER VIII

## PERIODICITIES (*continued*)

Lunar Periodicities. Lunations. Milne's Catalogue of Japanese Earthquakes. Analysed in Terms of Months. Author's Analysis. Imamura's Later Work. De Ballore's Analysis. Author's Examination of Lunar Day Periodicity. Omori's Investigations. Oldham's Discussion of Aftershocks of Assam Earthquake. Solar Day Periodicities. Davison's Analysis. Omori's Investigation of the Fluctuations in Aftershocks.

In 1893, Professor Milne published an important catalogue of Japanese earthquakes. These, numbering 8331 in all, were conveniently arranged according to districts, and I was tempted to investigate possible lunar periodicities among them. The labour involved in such an investigation is considerable, that is, if we are to group the material day by day throughout a whole lunar month, and not simply according to the moon's quadrantal positions.

As already stated, Perrey found evidence that earthquakes were more frequent at new moon and full moon than at half moon; more frequent at new moon than at full; more frequent when the moon was in perigee than when in apogee; more frequent at times of meridian passage of the moon than at other times. Later discussions along Perrey's lines have not corroborated his conclusions. His method was faulty inasmuch as it assumed that the maximum, if present, should occur at the lunar times mentioned. The only sure way of tracing a possible periodicity is to group the earthquakes day by day throughout the whole lunar month. If they were grouped even week by week, an existing periodicity might quite possibly be masked by the process of taking the means in successive weeks. On the other hand, by taking overlapping means over a submultiple of the whole period we are more certain of finding an existing periodicity of long period.

This is the method I adopted in a paper published in abstract in the Proceedings of the Royal Society of London in 1897, in which I discussed Milne's statistics of 8331 Japanese earthquakes from 1886 to 1892 inclusive. The following extract from the paper will sufficiently explain the nature of the investigation into the possible lunar-monthly and fortnightly periodicities.

'There are five distinct kinds of months recognized by astronomers, namely :—

(1) The anomalistic month (27·545 days).
(2) The tropical month (27·322 days).
(3) The synodic month (29·531 days).
(4) The sidereal month (27·3228 days).
(5) The nodical month (27·212 days).

'Of these, the last two cannot be regarded as having any influence on earthquake frequency, for the only conceivable effect is a tidal one, and the sidereal and nodical months have no necessary tidal relations. At the same time the periods of the sidereal and tropical months are so nearly the same that they can hardly be discriminated in the lapse of eight years. On the other hand, the anomalistic month may show itself in earthquake frequency, since the moon in perigee has a greater tidal action than when it is in apogee. Again, because of the moon's variation in declination, being now north of the Equator, now south, we may reasonably search for a tropical monthly periodicity. And, finally, the synodic or common month may make itself apparent, there being possibly a greater tidal stress when the moon is in syzygy (as in ordinary spring tides) than when the moon is in quadrature (as in neap tides).

'The earthquakes were accordingly tabulated according to these four months, whose periods differ appreciably; the nodical month being also included. For, by analysing the statistics in terms of both the tropical and nodical months, we may be the better able to draw conclusions as to the real existence of one or other periodicity.

. . . . . . . . . . . .

'It should be mentioned . . . that the number of earthquakes which really occurred during the last time interval was increased in the proper ratio; so that the frequency during this last interval was made comparable with the frequencies of the other intervals.

'In all cases the obvious aftershocks of any earthquake occurring on the same day were neglected. The 3000 after-

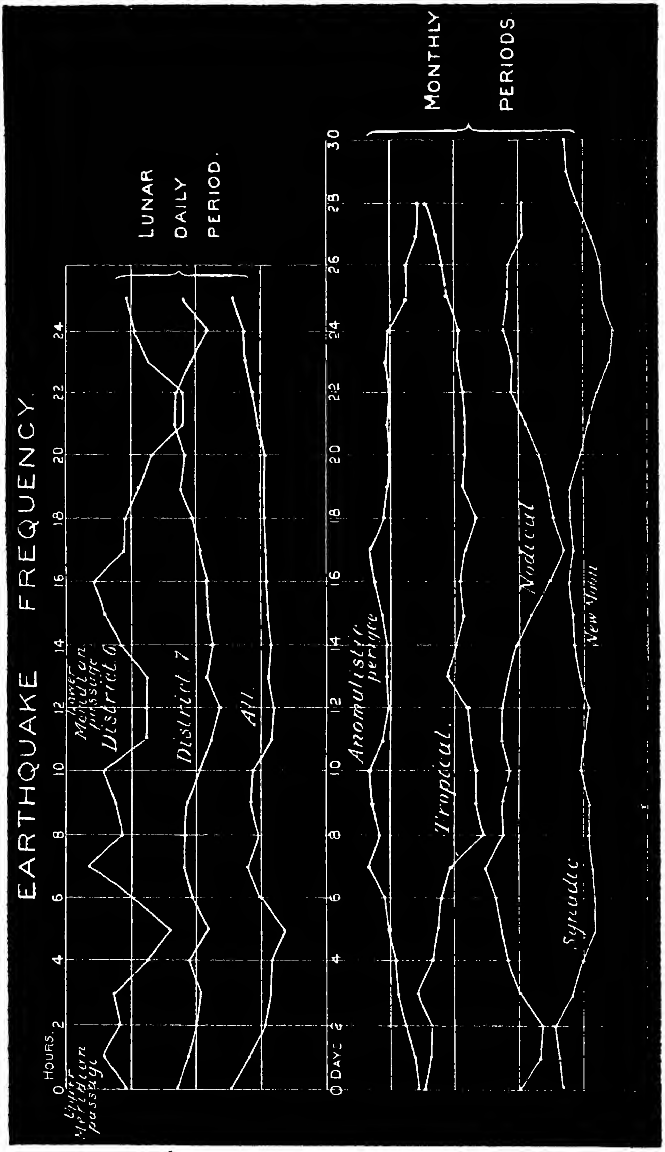



FIG. 28. LUNAR PERIODICITIES.
*(Reproduced by permission from Proc. R. S., 1897.)*

shocks of the great disaster of October 28, 1891, were also left out.

'The earthquakes on which the discussion is based numbered from 4725 to 4741, the number varying slightly for each monthly period, since, at the beginning and end of the eight years' interval, there were always a few, differing for the different months, which did not make up a complete period, and were, consequently, neglected.

'Each series of numbers was analysed harmonically as far as the first four harmonics, &c.'

The statistics on which the investigation was based were not given in the published abstract. They are tabulated in the following table, which shows the daily frequency throughout each month.

LUNATION FREQUENCIES

| Day | Anomalistic | Tropical | Nodical | Synodic |
|---|---|---|---|---|
| 1 | 153 | 183 | 150 | 168 |
| 2 | 152 | 198 | 182 | 173 |
| 3 | 174 | 173 | 166 | 176 |
| 4 | 155 | 188 | 156 | 169 |
| 5 | 204 | 220 | 213 | 139 |
| 6 | 157 | 150 | 180 | 145 |
| 7 | 168 | 183 | 203 | 142 |
| 8 | 186 | 163 | 176 | 177 |
| 9 | 196 | 158 | 187 | 170 |
| 10 | 180 | 130 | 162 | 155 |
| 11 | 179 | 172 | 186 | 155 |
| 12 | 173 | 185 | 188 | 160 |
| 13 | 151 | 176 | 190 | 174 |
| 14 | 178 | 164 | 184 | 157 |
| 15 | 182 | 189 | 153 | 176 |
| 16 | 185 | 155 | 164 | 158 |
| 17 | 181 | 166 | 147 | 161 |
| 18 | 174 | 179 | 134 | 184 |
| 19 | 188 | 157 | 148 | 149 |
| 20 | 150 | 156 | 186 | 181 |
| 21 | 175 | 184 | 167 | 158 |
| 22 | 175 | 164 | 181 | 134 |
| 23 | 176 | 177 | 170 | 168 |
| 24 | 183 | 166 | 181 | 133 |
| 25 | 165 | 170 | 180 | 143 |
| 26 | 163 | 181 | 196 | 148 |
| 27 | 131 | 192 | 166 | 161 |
| 28 | 178 (97) | 190 (61) | 170 (36) | 173 |
| 29 | | | | 155 |
| 30 | | | | 181 (96) |
| Totals | 4731 | 4740 | 4732 | 4725 |

The number in brackets after the last tabulated frequency in each column is the real observed frequency during the fraction of the day which completes the corresponding month. The bracketed numbers are used in finding the total number of shocks. The curves shown in Fig. 28 are smoothed by taking overlapping means for five days.

The statistics were treated by the method of overlapping summations so as to accentuate the annual monthly and half-monthly periodicities. The results are given in the following table :—

LUNATION PERIODICITIES [1]

| Month | Day of Maximum | | Amplitude | | Expectancy |
|---|---|---|---|---|---|
| | Month | Half-month | Month | Half-month | |
| Anomalistic | 11–12 | 8 | ·0557 | ·0501 | ·0258 |
| Tropical . | 26–27 | 1 | ·0663 | ·0551 | ,, |
| Nodical . . | 8–9 | 11 | ·0602 | ·0705 | ,, |
| Synodic . . | 14 | 15 | ·0255 | ·0695 | ,, |

To quote from the paper of 1896, a study of this table 'discloses the presence of certain features which have no *raison d'être* on any rational theory of tidal stresses. The most important of these is the fact that the nodical month (that is, the passage of the moon from node to node), which has no direct connexion with tidal stress periodicity, is characterized by amplitudes greater on the average than those corresponding to the other months'. There is also, as Schuster pointed out, the question as to the relative magnitudes of the amplitudes and the expectancy or probable value of the amplitude if the events were quite disconnected and subject to no periodic law. The proba-

[1] I give here in a footnote the table of amplitudes and phases obtained from the same set of data by use of Fourier's analysis. The amplitudes are relatively very similar; but the phases differ considerably.

Amplitudes, $c$, and Phases, $a$, of the First Four Harmonics

| 'Month' | $c_1$ | $c_2$ | $c_3$ | $c_4$ | $a_1$ | $a_2$ | $a_3$ | $a_4$ |
|---|---|---|---|---|---|---|---|---|
| Anomalistic | 46·2 | 47·8 | 12·9 | 16·5 | 21·7 | 8·5 | 5·2 | 6·2 |
| Tropical . | 54·7 | 40·7 | 23·1 | 17·2 | 6·0 | 1·9 | 7·9 | 2·4 |
| Nodical . . | 49·5 | 55·2 | 28·3 | 17·6 | 1·2 | 7·9 | 6·9 | 2·7 |
| Synodic . . | 11·0 | 52·1 | 24·5 | 4·7 | 13·7 | 2·7 | 7·7 | 0·6 |

bility that the amplitude in such a case should exceed twice the expectancy is ·0432, or 1 in 23.

In the course of eight years there are 106 anomalistic lunations, 107 tropical, 107·4 nodical, and 98·9 synodic. The difference between the tropical and nodical should have been sufficient to get rid of a possible 'tropical' periodicity from the statistics when grouped according to the nodical month. But the amplitudes are more conspicuous in the nodical than in the tropical grouping. There seems to be therefore no distinct evidence of seismic frequency being dependent on variation of tidal stress as the moon approaches to or recedes from the earth, or as the moon passes from south of the equator to north of the equator and back again.

As regards the synodic month, the greatness of the half-monthly amplitude as compared with the monthly amplitude, and the times of occurrence of the maxima and minima, lend some support to the view that we have an indication of tidal effect. The maxima occur at the times of new moon and full moon, just about the times when, in virtue of the combined action of sun and moon, the ocean tides attain their maxima.

In No. 18 of the Publications of the (Japanese) Earthquake Investigation Committee (1904), Mr. Imamura has discussed in considerable detail the synodic monthly periodicities of the seismic frequencies of the same groups of earthquakes which Omori had already utilized in his work on the annual and semi-annual periodicities. Here also in the great majority of cases the statistics are too meagre and the lapse of time too short for any certain conclusion to be drawn. The method of analysis employed by Imamura is identical with that used by Omori, and is purely graphical. The shocks are first tabulated in lunar days, reckoning from new moon; and the total synodic daily numbers so obtained are plotted on section paper, the successive days of the lunar month being laid off horizontally. The very irregular graph so obtained is then smoothed by 'drawing a continuous free-hand line, which passes through the mean positions of every two consecutive

points tangentially to the (straight) line connecting directly the points themselves'. The varieties of graph are nearly as numerous as the places for which they are drawn. In all cases there are from ten to fifteen crests and hollows of varying amplitudes; but it cannot be said that there is a tendency for the crests of greatest amplitude to occur at the same time for even the majority of the stations. For example, there is a prominent crest about day 17 in the graphs for Nagano, Fukuoka, Niigata, Tokyo, Maebashi, Mito, and Nemuro, that is, practically all over the country from the extreme south-west to the extreme north-east; but in the graphs for Kagoshima, Gifu, Osaka, Nagoya, Wakayama, Aomori, Hikone, Hakodate, Fukushima, and Ishinomaki, the prominent crests occur at quite other times, and in three important cases there is absolutely no crest at all within two days of that date. Imamura considers that he finds evidence of the existence of four principal maxima throughout the lunar month. Two of these he connects with the times of opposition and conjunction of sun and moon; and the other two he connects in a very ingenious, but in my opinion very artificial and forced manner with the concurrence of the times of daily barometric maximum and ordinary high tide. It is obvious that with crests occurring on an average every two or three days it is not difficult to find four such coming within a day or two of particular dates; and having regard to the meagreness of the statistics in most instances, and the doubtful accuracy of the method of smoothing, I question if any confidence can be placed on the results indicated. To show the danger of the method of smoothing I need only refer to the case of Mito, which was mentioned above as having a pronounced crest near day 18. By a second process of smoothing this prominent crest practically disappears, and in one of the tables the dates for the four maxima are given as days 6, 14, 26, and 30.

In addition to the graphs for recent earthquakes in the individual stations, Imamura gives also the graphs for the historic shocks (416 A.D. to 1860 A.D.) of Japan and Kyoto. The former catalogue includes 2525 shocks and the latter

1604. The graphs are fairly similar, showing, after one smoothing, pronounced maxima on days 2, 7, 13, and 24. The Tokyo graph based on 2464 shocks between 1875 and 1902 has its maxima falling on the days 7, 18, 23, and 30. The maxima on days 7 and 23 agree very well with the 7 and 24 of the Japan graph ; but these are connected with coincidence of causes which, although admissible for a limited region like Tokyo, cannot possibly be applied to the whole of Japan. It cannot be said that 30 and 18 are in good agreement with 2 and 13, there being a difference of three days between the first pair and five between the second; and yet these, being by hypothesis due to conjunction of sun and moon, ought to be in good agreement all over the country.

So far as the number of shocks involved are concerned the four best catalogues prepared by Imamura are those for Tokyo, Gifu, Ishinomaki, and Miyako. These, along with the historic list for all Japan, I have submitted to the method of overlapping means, and have obtained the following results for the assumed monthly and half-monthly periodicities.

Synodic Month Periodicities

| | No. of Shocks | Day of Maximum | | Amplitude | | Expectancy |
|---|---|---|---|---|---|---|
| | | Month | Half-month | Month | Half-month | |
| Japan . . | 2525 | 8 | 4 | ·079 | ·044 | ·0354 |
| Tokyo . . | 2464 | 23 | 5 | ·066 | ·066 | ·0356 |
| Gifu . . . | 938 | 10 | 6 | ·077 | ·050 | ·0579 |
| Ishinomaki. | 1307 | 19 | 13 | ·112 | ·065 | ·049 |
| Miyako . . | 923 | 3 | 10 | ·126 | ·137 | ·0583 |

With the view of testing directly the suggested existence of four maxima at times of conjunction and times of quadrature, I divided Imamura's numbers for Tokyo into half-monthly sets, making sixty in all. These were then divided into four groups of 15 each, and finally combined into one resultant set of 15, each member of this resultant set being the sum of the four numbers occupying the corresponding places in the initial groups. This final group was then subjected to the usual treatment by overlapping summa-

tions so as to emphasize the period of 7·5 days. The maximum came out about the end of the second day, recurring of course at intervals of 7·5 days thereafter. The corrected amplitude was 0·038, which is very slightly greater than the expectancy. The times of maxima indicated by this method of analysis, namely, 2·5, 10, 17·5, and 25, do not agree particularly well with Imamura's deductions from the smoothed graphs, namely, 30, 7, 18, 23. The evidence in favour of the existence of a quarter-monthly periodicity seems to be much more precarious than the evidence in favour of the existence of the longer associated periodicities.

If we take along with these five sets of statistics the set prepared by myself from Milne's catalogue, we find very little harmony among them. There is absolutely no agreement in the times of the maximum frequencies either for the monthly or half-monthly periodicities. The amplitudes are no doubt of the same order of magnitude, but in no case more than three times the expectancy. In Imamura's statistics there is no indication of the pronounced half-monthly amplitude which was characteristic of the larger mass of statistics used by me. On the whole we are driven to the conclusion that the reality of a tidal periodicity in earthquake periodicity due to the action of the moon is still unproved.

We pass now to the consideration of periodicities of comparatively short period, namely, the lunar and solar days. Let us consider the lunar day first.

Perrey concluded from his statistics that earthquakes were more frequent at times of meridian passage of the moon than at other times. No serious attempt was made to test this conclusion with the increased and yearly increasing material to hand till 1889, when de Ballore with his catalogue of nearly 45,000 shocks found no clear evidence at all for a lunar daily periodicity. Dividing the whole lunar day of 24 h. 50 m. into eight parts, of which the middle of the first part corresponds to the time of the superior culmination, he found the following grand total of earthquake distributions :—

| Eighths | I | II | III | IV | V | VI | VII | VIII |
|---|---|---|---|---|---|---|---|---|
| Earthquakes | 5579 | 5558 | 5611 | 5508 | 5802 | 5564 | 5571 | 5662 |

If we treat these by the method of overlapping sums so as to separate out the daily and half-daily periods we find the daily maximum to fall exactly at the time of the lower culmination, and the half-daily maxima at the times very slightly preceding the times of the superior and inferior culminations. The corrected amplitude comes out 0·0125 for the daily period and 0·01 for the half-daily, while Schuster's expectancy for an equal number of random events is ·0084. The probability that the amplitude for the random events should exceed 0·0125 is 0·172 or about 1 in 6. If we accept the result as evidence that the daily tidal stress due to the moon has an influence on seismic frequency the effect is very slight.

In the paper on Lunar Periodicities already referred to I discussed also the lunar daily period in the statistics afforded by Milne's catalogue of Japanese earthquakes. The earthquakes in each of the fifteen districts into which Milne divided the country were tabulated in quarter-hour intervals after meridian passage of the moon. The statistics for the ten groups into which for convenience the fifteen were arranged were then discussed separately by Fourier's analysis as far as the fourth harmonic. In all but Districts 6 and 7 the numbers of shocks on which the discussion was based were too small to yield trustworthy results. This was shown by the largeness of the amplitudes as compared with the amplitudes furnished by the statistics for Districts 6 and 7.

Here we shall confine our attention entirely to the statistics of the two districts named, and the following table gives the data tabulated in quarter-hours throughout the lunar day.

We may call District 6 the Tokyo district, and District 7 the Nagoya district. The latter did not become prominently seismic till after the great shock of 1891.

In Fig. 28 (p. 132) smoothed curves of the lunar daily periods are shown for Districts 6 and 7, and for all combined.

| Hour | Tokyo District | | | | Nagoya District | | | |
|---|---|---|---|---|---|---|---|---|
| 1 | 19 | 11 | 18 | 17 | 29 | 37 | 43 | 33 |
| 2 | 10 | 17 | 9 | 21 | 44 | 34 | 41 | 49 |
| 3 | 13 | 17 | 20 | 12 | 28 | 45 | 33 | 38 |
| 4 | 13 | 18 | 6 | 8 | 30 | 41 | 29 | 39 |
| 5 | 15 | 19 | 15 | 16 | 30 | 22 | 39 | 38 |
| 6 | 19 | 6 | 11 | 14 | 38 | 31 | 40 | 43 |
| 7 | 12 | 11 | 13 | 11 | 28 | 30 | 37 | 35 |
| 8 | 18 | 24 | 24 | 13 | 34 | 46 | 39 | 42 |
| 9 | 6 | 25 | 18 | 16 | 46 | 29 | 34 | 39 |
| 10 | 6 | 20 | 12 | 11 | 41 | 38 | 30 | 19 |
| 11 | 21 | 12 | 9 | 12 | 28 | 45 | 33 | 44 |
| 12 | 17 | 10 | 12 | 13 | 39 | 31 | 33 | 34 |
| 13 | 12 | 18 | 13 | 17 | 28 | 47 | 35 | 36 |
| 14 | 16 | 17 | 20 | 12 | 31 | 35 | 32 | 42 |
| 15 | 12 | 10 | 10 | 17 | 35 | 37 | 37 | 32 |
| 16 | 13 | 14 | 16 | 22 | 36 | 30 | 33 | 43 |
| 17 | 11 | 14 | 20 | 14 | 42 | 36 | 33 | 34 |
| 18 | 14 | 19 | 16 | 15 | 36 | 31 | 37 | 40 |
| 19 | 13 | 15 | 10 | 15 | 40 | 36 | 30 | 42 |
| 20 | 11 | 11 | 10 | 16 | 37 | 40 | 41 | 41 |
| 21 | 18 | 11 | 11 | 19 | 38 | 38 | 44 | 37 |
| 22 | 13 | 20 | 17 | 4 | 42 | 33 | 34 | 37 |
| 23 | 9 | 12 | 19 | 10 | 34 | 43 | 37 | 40 |
| 24 | 15 | 8 | 16 | 14 | 39 | 41 | 34 | 31 |
| 25 | 17 | 23 | 10 | 13 | 31 | 32 | 44 | 25 |
| | Total | | | 1432 | | | | 3632 |

From these numbers we may by suitable grouping and taking of overlapping sums deduce the daily, half-daily, and quarter-daily periodicities. They are as follows :—

| Lunar Day | Amplitude | | Hour of Maximum after culmination | |
|---|---|---|---|---|
| | Tokyo | Nagoya | Tokyo | Nagoya |
| Whole . . | 0·048 | 0·029 | 12 | 22 |
| Half . . . | 0·062 | 0·045 | 2–3 | 8 |
| Quarter . . | 0·059 | 0·055 | 3 | 2 |
| Expectancy | 0·047 | 0·029 | | |

The times of maximum are quite different in the two cases, although the two districts do not lie very far from each other. It is not easy to see why there should be such a marked difference if there be anything of the nature of a tidal stress affecting the seismic frequency. Moreover, in accordance with Schuster's criticism the amplitudes are for the daily periodicity almost identical with the expect-

ancy for haphazard events. There is consequently no evidence of a lunar daily period. The half and quarter-daily amplitudes are somewhat larger; and the fact that this occurs in both sets of statistics may be regarded as hinting at the possibility of such a periodicity; but clearly we have not yet accumulated a sufficient amount of material over a limited region of the earth to warrant us in drawing any but a negative conclusion.

Omori has studied the daily lunar period in connexion with the earthquake frequencies at Nagoya, Nemuro, and Tokyo, the number of shocks and interval of time being as follows:—

| | | | | |
|---|---|---|---|---|
| Nagoya | 1270 | earthquakes | from | 1891 to 1899 |
| Nemuro | 799 | ,, | ,, | 1894 to 1899 |
| Tokyo | 1462 | ,, | ,, | 1888 to 1899 |

Treating the numbers by the method of overlapping sums I find the following results for the times of the lunar daily, half-daily, and quarter-daily maxima, and for the corresponding corrected amplitudes. The reckoning is from upper culmination:—

| | Nagoya | Nemuro | Tokyo | All | All (Solar) |
|---|---|---|---|---|---|
| | Hour | Hour | Hour | Hour | Hour |
| Daily max. | 7·5 | 7·5 | 10·5 | 7·5 | 3·5 |
| Half-daily max. | 3·5 | 2·5 | 3·5 | 3·5 | 2·5 |
| Quarter-daily max. | 4 | 1·5 | 5·5 | 3 | 5 |
| | Amp. | Amp. | Amp. | Amp. | Amp. |
| Daily | 0·095 | 0·065 | 0·016 | 0·048 | 0·081 |
| Half-daily | 0·112 | 0·095 | 0·032 | 0·081 | 0·016 |
| Quarter-daily | 0·05 | 0·013 | 0·067 | 0·017 | 0·05 |
| Expectancy | 0·05 | 0·063 | 0·046 | 0·03 | 0·03 |

These results do not at all agree with the conclusions arrived at by Omori, who does not work with overlapping means, but seems to assume that the hour with the greatest observed frequency is the hour of maximum frequency. But this could only be true when a very great number of shocks were taken into account. Omori considers also what he calls three-hourly distributions. The frequencies for the hour intervals 1–3, 4–6, 0–3, 3–6, 6–9, 9–12, 12–15, 15–18, 18–21, 21–24, are added together forming eight

separate summations, and these are then considered as indicating certain periodicities. The fallacy of this method lies in the fact that there is no reason why this particular grouping should be chosen instead of any other similar grouping, such as, for example, 2–5, 5–8, 8–11, and so on to the last 23–2. But when this is done it will be found that in the case (to take simply the first) of Nagoya, the principal minimum number of this three-hourly distribution falls between the 17th and 20th hours instead of the 21st and 24th hours according to the distribution used by Omori. The truth is that any arbitrary method of partitioning the numbers in separate and non-overlapping subgroups may very easily be such as to mask a real periodicity instead of accentuating it relatively to other possible periodicities.

On the other hand, if there be any real daily, half-daily, or quarter-daily periodicity present, the method of taking overlapping sums over suitable intervals cannot fail to bring them into evidence. In the present condition of earthquake statistics this seems to me to be the only reasonable method for testing the existence of possible periodicities. The labour involved in applying the Fourier analysis to insufficient statistics is a sheer waste of time.

Let us now examine the results as a whole. The first point to notice is the tendency for the relative amplitudes to diminish as the statistical number increases. This shows that no reliance can be placed on the values of amplitudes unless the number of shocks is very great—greater than any of the individual cases discussed by Omori, Imamura, or myself. Taking into account de Ballore's much larger body of statistics, the only result on which most of the cases agree is the tendency for the half-daily maxima to occur in the neighbourhood of times of culmination. This might be considered as due to the daily tidal stress of the moon.

The last column of the table on p. 141 gives corresponding results for the solar instead of the lunar day. They are inserted for the sake of comparison and will be referred to further on, when the question of the solar day is considered.

The lunar day periodicity has been considered in a novel

way by R. D. Oldham with reference to the aftershocks of the great Assam earthquake of June 12, 1897.[1] An interesting discussion is given of the character of the tidal stresses produced by sun and moon as it is influenced by the declination of these bodies. This leads to the grouping of the statistics in three sets according as the declination is greater than 9° N., lies between 9° N. and 9° S., or is greater numerically than 9° S. The grouping is effected with reference both to the lunar and the solar day. The lunar day is taken as equal to 25 mean solar hours, an appropriate correction being applied to the frequency as obtained for the last hour. Unfortunately it is not possible to apply at once the method used systematically throughout this chapter to statistics which are arranged in an odd number of groups. Accordingly I have slightly readjusted the numbers given so as to arrange them in 24 groups, increasing the numbers as given by Oldham by a suitable proportion, but so as to leave the whole number of earthquakes the same. The method may be open to criticism, but the rearrangement so deduced cannot differ appreciably from what would have been obtained had the statistics been arranged *de novo* in groups of 24 to the lunar day. Applying to this rearrangement and to the solar-day statistics the method of overlapping sums I obtained the two following tables of results. The solar day periodicity is to be considered in the succeeding paragraphs; but it is convenient to give the two arrangements of the same set of statistics together so that they may be easily compared.

Lunar Day Periodicities

| Declination | Number of earthquakes | Day | Half-Day | Quarter-Day | Expectancy |
|---|---|---|---|---|---|
| 9° N.+ | 510 | 18 h.<br>·099 | 4 h.<br>·090 | 3·5 h.<br>·099 | ·078 |
| 9° N. to 9° S. | 333 | 18 h.<br>·147 | 8 h.<br>·308 | 5·5 h.<br>·099 | ·097 |
| 9° S.+ | 431 | 24 h.<br>·202 | 1 h.<br>·128 | 0·5 h.<br>·113 | ·085 |
| All | 1274 | 20 h.<br>·062 | 8 h.<br>·05 | 1·5 h.<br>0·4 | ·05 |

[1] See *Memoirs of the Geological Survey of India*, vol. xxxv, part 2.

SOLAR DAY PERIODICITIES

| Declination | Number of earthquakes | Day | Half-Day | Quarter-Day | Expectancy |
|---|---|---|---|---|---|
| 9° N.+ | 436 | 17 h. ·137 | 9 h. ·152 | 4 h. ·137 | ·083 |
| 9° N. to 9° S. | 358 | 18·5 h. ·132 | 6 h. ·132 | 4·5 h. ·255 | ·094 |
| 9° S.+ | 480 | 1 h. ·335 | 5 h. ·118 | 5·5 h. ·092 | ·087 |
| All | 1274 | 1 h. ·177 | 9 h. ·076 | 4·5 h. ·144 | ·05 |

The first column gives the declination of the tide-producing body; the second the number of earthquakes; and the last column the expectancy. The third, fourth, and fifth columns contain the phases and amplitudes calculated in the way used throughout the chapter. The number entered in the phase sub-column is the time of maximum reckoning from the lower culmination of the tide-producing body, that is, from midnight in the case of the solar day, and from what might be called mid-lunar-night in the other case.

From theoretical considerations Oldham was led to look for maximum frequency about the time of higher culmination when the influencing body was well to the north of the equator and about the time of lower culmination when the body was well to the south of the equator. There is some hint of this in both tables; but the evidence of the amplitude is not reassuring. For example, if the moon's tidal stress were to have any influence, we should expect to find the amplitudes as large for declinations greater than 9° N. as for declinations greater than 9° S. Not only is this not the case but the values of the amplitudes in most cases are not much greater than the expectancies. Although it must be admitted that the results so far are negative, nevertheless the point of view which led Oldham to discriminate between cases according to the declination is one deserving of more attention. It emphasizes the possibility, already referred to in other connexions, that in an indiscriminate grouping according to probable periodicities there may be a balancing of effects, so that the periodic change looked for may escape notice.

The idea that earthquakes happen more often during the night than during the day is pretty general in earthquake countries. From his investigations in Japan Milne early came to the conclusion that the apparent preponderance of night over day shocks was really a personal equation of the observers, who are better able to appreciate a moderate earthquake during the still night than during the noisy day. Instrumental records declared no night maximum.

In his paper of 1889 already referred to M. de Ballore considered the question as to how far his statistics of 45,000 earthquakes lent support to the hypothesis of a diurnal period in seismic frequency. I quote from an abstract of his paper which I prepared for the Seismological Society of Japan (see *Trans. S. S. J.*, 1890).

'In five of the seven groups into which, for sound scientific reasons, he divided the shaken regions, the numbers and curves alike showed a night maximum and a day minimum. The grand mean for the first six groups, including 37,511 earthquakes, showed that the ratio of the day to the night earthquakes was as 8 : 10. By far the great majority of these earthquakes were not instrumentally recorded; so that the problem was really as much one of human sensitiveness to seismic influence as one of earthquake frequency. M. de Ballore concluded in fact that this ratio of 8 to 10 represented the relative loss of day earthquakes caused by the physiological conditions of human activity.

'In Group VII, which was excluded in the above grand mean, a distinct preponderance of day earthquakes was shown. But Group VII consisted of the Italian geodynamic stations, at which delicate self-recording seismographs were in use, working day and night. A large number of small tremors were registered, in which real seismic disturbances were probably mingled with motions due to human operations. Separating out the smallest disturbances, those namely of Grade I on the Rossi-Forel scale of intensities, we get a marked diminution in the apparent day preponderancy.

'Then, again, an instructive corroboration of the conclusion given above was obtained by a discussion of the earthquakes in intensity groups. These are ten in number, according to the Rossi-Forel conventional scale; No. I being the small instrumental ones just mentioned, and No. X

being disasters. The ratios of the day and night earthquakes for the different intensities were as follows :—

| Intensity | I | II | III | IV | V | VI | VII | VIII | IX | X |
|---|---|---|---|---|---|---|---|---|---|---|
| Ratio of day to night | 1·8 | ·73 | ·60 | ·67 | ·65 | ·76 | ·81 | ·85 | 1·27 | 1·02 |

'Thus we see at a glance that it is for Intensities III, IV, and V, that the night maximum is specially obvious. Now these intensities are just the intensities that would be apt to be missed during the noisy day. The apparent day maximum for earthquakes of Intensity I has been already explained—these being all registered by delicate seismographs. In the higher intensities there is a tendency to equality—just as we should expect, since emphatic earthquakes will be as evident during the day as during the night.

'So far, then, we have no evidence in support of the somewhat prevalent notion that earthquakes love the darkness rather than the light.'

This dichotomous division of shocks into day and night groups does not, however, dispose of the question as to the existence of a diurnal periodicity. For this purpose we must arrange the shocks in small time intervals, most conveniently in hours, and then discuss the statistics in some systematic manner.

In an important paper on the Aftershocks of Earthquakes,[1] Omori has grouped certain sets of shocks in hour intervals with the view of testing any existing periodicity. The results were represented graphically, and the graphs were smoothed apparently by the same process as that subsequently used by him in treating of the annual and lunar monthly periodicities. The method is somewhat arbitrary and leads to no true numerical relation among the amplitudes or phases of the possible periodicities.

In a paper published in the *Philosophical Magazine* (1896) Dr. Davison applied harmonic analysis to certain earthquake statistics, carrying the determination of the constituents as far as the sixth harmonic.

For the sake of uniformity I propose to discuss by the much simpler method of overlapping sums some of the statistics already discussed by Omori and Davison. It is

[1] *Journal of the College of Science, Imperial University, Japan*, vol. vii, 1894.

not necessary here to reproduce the statistics of all the cases treated; but to show their nature it will suffice to give two or three extreme cases. The number of shocks in each hour interval is entered in the column headed by the corresponding hour. The first row in each case refers to a.m. and the second row to p.m. In the subsequent discussion the day is reckoned from midnight and 1 p.m. is called 13 and so on.

| *Hour* | 0–1 | 1–2 | 2–3 | 3–4 | 4–5 | 5–6 | 6–7 | 7–8 | 8–9 | 9–10 | 10–11 | 11–12 | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *District* | | | | | | | | | | | | | |
| Italy<br>1872–87 | 264<br>452 | 238<br>388 | 264<br>443 | 250<br>454 | 248<br>385 | 257<br>347 | 260<br>294 | 308<br>276 | 407<br>346 | 482<br>318 | 527<br>286 | 436<br>247 | a.m.<br>p.m. |
| Japan<br>1885–90 | 133<br>151 | 153<br>186 | 211<br>188 | 176<br>162 | 139<br>144 | 166<br>132 | 153<br>135 | 162<br>156 | 151<br>153 | 157<br>184 | 139<br>185 | 155<br>171 | a.m.<br>p.m. |
| Gifu<br>13 days<br>1891 | 56<br>54 | 64<br>48 | 58<br>38 | 62<br>36 | 92<br>36 | 63<br>47 | 47<br>51 | 53<br>77 | 49<br>58 | 41<br>51 | 42<br>42 | 58<br>35 | a.m.<br>p.m. |
| Kumamoto<br>14 days<br>1899 | 7<br>11 | 13<br>5 | 18<br>2 | 12<br>1 | 11<br>4 | 11<br>3 | 3<br>8 | 6<br>4 | 4<br>5 | 2<br>2 | 4<br>4 | 3<br>5 | a.m.<br>p.m. |

The Italian numbers refer to tremors recorded on Tromometers; and probably mechanical tremors due to traffic may be mixed up with true seismic tremors. This would tend to increase during the day. The Japan list contains instrumentally observed earthquakes of small intensity. The Gifu numbers are the aftershocks for a fortnight after the great Mino-Owari earthquake of October 28, 1891; and the Kumamoto numbers are aftershocks of the earthquake of July 28, 1889.

It is evident at a glance that these several groups have very different claims upon our attention. The inadequacy of meagre statistics in a limited region to serve as a basis for calculation will be apparent in the results now to be given. Although I have no belief in the reality of an eight-hours period I have worked out the phase and amplitude for this period and for an assumed four-hours period as well as for the day, half-day, and quarter-day, the reasons being (1) because Omori lays a good deal of emphasis on

the existence of the eight-hours period, (2) because, in my opinion, a large eight-hours amplitude proves the insufficiency of the statistics to establish any frequency.

The eight-hours period was investigated by the same method as that used for the half-daily and quarter-daily periods. The statistics were arranged in groups of eight hours. That is, hours 1, 9, 17 were taken together; also 2, 10, 18; 3, 11, 19; and so on. The numbers so obtained were then discussed by taking overlapping sums over four hours.

The results are shown in the following table. The first column contains the names of the countries or districts, the second the number of shocks, and the third the expectancy. The five remaining pairs of columns give the amplitudes and the phases or times of maximum for the various periodicities considered. Only the *earliest* hour of maximum is entered for the higher periodicities. In the first column the interval of time through which the statistics were taken is also given.

AMPLITUDES AND PHASES OF ASSUMED DAILY AND SUB-DAILY PERIODICITIES

| District | Number of Shocks | Expectancy | 24-hours | 12-hours | 6-hours | 8-hours | 4-hours |
|---|---|---|---|---|---|---|---|
| Italy<br>1872–87 | 8177 | 0·02 | 0·130<br>13 h. | 0·118<br>11 h. | 0·129<br>3 h. | 0·05<br>1 h. | 0·019<br>1 h. |
| Japan<br>1885–90 | 3842 | 0·029 | 0·054<br>2 h. | 0·076<br>2 h. | 0·098<br>25 h. | 0·065<br>6 h. | 0·018<br>3 h. |
| Tokyo<br>1876–91 | 1168 | 0·052 | 0·137<br>18 h. | 0·059<br>10 h. | 0·115<br>3 h. | 0·142<br>6 h. | 0·037<br>1 h. |
| Gifu<br>13 days<br>1891 | 1258 | 0·050 | 0·206<br>24 h. | 0·106<br>6 h. | 0·062<br>1 h. | 0·219<br>4 h. | 0·11<br>4 h. |
| Nagoya<br>13 days<br>1891 | 572 | 0·074 | 0·35<br>3 h. | 0·19<br>3 h. | 0·049<br>2 h. | 0·13<br>2 h. | 0·18<br>4 h. |
| Chiran<br>5 months<br>1893–4 | 233 | 0·116 | 22<br>0·16<br>4 h. | 0·164<br>4 h. | 0·164<br>2 h. | 0·097<br>3 h. | 0·25<br>2 h. |
| Kumamoto<br>1 month<br>1889 | 148 | 0·146 | 0·51<br>2 h. | 0·52<br>3 h. | 0·279<br>1 h. | 0·435<br>4 h. | 0·088<br>1 h. |
| Manila<br>1869–89 | 208 | 0·123 | 0·32<br>11 h. | 0·19<br>3 h. | 0·34<br>5 h. | 0·211<br>7 h. | 0·14<br>1 h. |

A comparison of the amplitudes with Schuster's 'expectancy' brings out some interesting points. In the case of Italy with its 8177 shocks the daily, half-daily, and quarter-daily amplitudes are all comparatively large, and to that extent support the hypothesis of a true solar daily period. On the other hand, the eight-hours and four-hours periodicities show small amplitudes, as on general grounds we should expect. The comparative smallness of these two amplitudes is not, however, shown in the other cases, at any rate so distinctly. It is important to note that when the values of the expectancy are large the amplitudes, though in most cases somewhat larger, are not markedly so. This would indicate that the statistics are too meagre to establish the existence of the daily period. The amplitudes for the Italian and Tokyo statistics are respectively 0·13 and 0·137; and the fact that a smaller body of statistics, as in the four last cases, is associated with a distinctly larger amplitude simply shows that the statistics are not numerous enough. In fact the expectancy in the cases of Chiran, Kumamoto, and Manila, is practically equal to the amplitudes which are given by the fuller statistics of Italy and Tokyo. Where the statistics are limited the haphazard law masks the true periodicity which is being searched for.

Where comparison is possible the results agree sufficiently well with the results given by Davison, at least as regards the phases of the assumed periodicities. With the material grouped in hour intervals we cannot look for any great accuracy in the determination of the times of maximum—to within half-an-hour perhaps but no nearer. Davison gives his phases or epochs to minutes, but that is a refinement quite beyond the possibilities of the investigation.

The Japan group of shocks from 1885 to 1890 is taken from Omori's list. The group analysed by Davison was compiled from Milne's great catalogue, and covers the same period. It includes only 1175 shocks, whereas Omori's list includes 3842. There must be included in the latter list a great many small shocks. Davison finds for the times of maximum in the daily, half-daily, third-daily, and fourth-daily harmonic the following values :—noon, 9 a.m.,

6.5 a.m., and 2.5 a.m.; while my values for the corresponding quantities as given by Omori's larger list are 2 a.m., noon, 6 a.m., and 3.5 a.m. There is agreement in one case only and that is the eight-hours periodicity, for the existence of which there is absolutely no physical basis. The daily and half-daily periodicities are quite different.

Again, if there were any demonstrable reality in the daily and half-daily periodicities we should expect Gifu and Nagoya to give similar results; for both are in the same seismic district and were subject to the aftershocks of the same great earthquake. But they differ emphatically until we reach the four-hours periodicity, of which also there is absolutely no imaginable physical cause. The Gifu shocks show a marked eight-hours period; the Nagoya shocks show practically none. This merely means that within the short compass of thirteen days certain marked irregularities occurred so as to bring out an eight-hours period. Indeed a close examination of Omori's list of Gifu aftershocks reveals the fact that this eight-hours period is determined by the first three of the thirteen complete days for which the statistics are given, and to a very great extent by the first of these three. The table of Gifu shocks arranged according to day and hour is given below (p. 152) mainly for another purpose; and a glance will suffice to show how important in shaping the character of the whole is the distribution during the early days.

To make the argument more complete I have separated out the statistics into two groups, the first containing the 627 of October 29, 30, and 31, the second the remaining 631 during the first ten days of November. Treating the data by the same method of overlapping sums I find the times of maximum and the reduced amplitudes as follows:—

| | 24-hours | 12-hours | 6-hours | 8-hours | 4-hours |
|---|---|---|---|---|---|
| Oct. 29–31 | h. 7<br>·30 | 3 or 6<br>·22 | 1·5, 7·5, &c.<br>·061 | 4, 12, 20<br>·43 | 4, 8, 12, &c.<br>·26 |
| Nov. 1–10 | h. 1<br>·21 | 8<br>·11 | 1·5, &c.<br>·11 | 5, 13, 21<br>·048 | 2, 6, 10, &c.<br>·053 |

Expectancy = ·071

Thus the eight-hours period which is so conspicuous in the statistics of the first three days, having an amplitude greater than that of even the daily or half-daily periodicity, is practically non-existent in the statistics of the last ten days, its amplitude being only one quarter of that of the daily period and one half that of the half-daily, and being besides less than the expectancy. Moreover, the times of the occurrence of the maxima in the daily and half-daily periodicities are quite different in the two cases and differ also from the corresponding quantities when the statistics are treated as a whole. In short, we can base upon these results no conclusions whatever. The data are too meagre.

The times of the daily maximum for the four stations Gifu, Nagoya, Chiran, and Kumamoto, all of which deal with aftershocks, occur within four hours of midnight, and one of the half-daily maxima occurs about the same time. This might have been accepted as evidence of a direct tidal action due to the sun; but unfortunately the same result is not given by the Italian shocks and by the Tokyo group analysed above. The Japan group given in the table agrees fairly well with the aftershock groups; but against this we must set Davison's analysis of another Japan group. The combined group of Nagoya, Nemuro, and Tokyo given in the table on p. 141 shows a diurnal and semi-diurnal periodicity in tolerable agreement also with the Japan group of the last table, but not at all in agreement with the Tokyo group. The evidence is, in short, distinctly conflicting; and it is very doubtful if we can place any reliance on conclusions (1) as to the existence of a periodicity connected with the succession of day and night, (2) as to the times of occurrence of maximum and minimum frequencies.

At Milne's suggestion Omori undertook the first systematic examination into the law of aftershocks of great earthquakes.[1] A good example is the case of Gifu, a town in the district of Japan visited by the disaster of 1891. The following table, reproduced from Omori's memoir, shows the hourly distribution for thirteen days after the earthquake.

[1] See *Introduction* to Milne's 'Catalogue of 8331 Earthquakes,' *Seismological Journal of Japan*, vol. iv, 1895.

Hourly Numbers of Earthquakes at Gifu from October 29 to November 10, 1891

| | October | | | | November | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Hour | 28 | 29 | 30 | 31 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | Sum |
| a.m. | | | | | | | | | | | | | | | |
| 0–1 | | 9 | 13 | 1 | 7 | 9 | 2 | 4 | 1 | 2 | 5 | 2 | | 1 | 56 |
| 1–2 | | 19 | 12 | | 9 | 1 | 2 | 6 | 3 | 2 | 1 | 2 | 4 | 3 | 64 |
| 2–3 | | 15 | 6 | 10 | 9 | 4 | 1 | 3 | 2 | 3 | | 1 | 1 | 3 | 58 |
| 3–4 | | 13 | 20 | 5 | 1 | 7 | 3 | 2 | 3 | 3 | 1 | | 2 | 1 | 61 |
| 4–5 | | 25 | 20 | 12 | 3 | 6 | 6 | 2 | 1 | 7 | 2 | 2 | 6 | | 92 |
| 5–6 | | 15 | 8 | 13 | 4 | 2 | 3 | 4 | 4 | 3 | 1 | 2 | 2 | 2 | 63 |
| 6–7 | | 4 | 2 | 7 | 7 | 2 | 3 | 5 | 2 | 2 | 4 | 1 | 5 | 3 | 47 |
| 7–8 | | 14 | 5 | 6 | 4 | 4 | 5 | 2 | 3 | 1 | 2 | 3 | 1 | 3 | 53 |
| 8–9 | | 14 | 9 | 7 | 2 | 1 | 5 | 2 | 2 | | 3 | 2 | 1 | 1 | 49 |
| 9–10 | | 9 | 4 | 4 | 2 | 4 | 2 | 5 | 3 | 2 | 2 | 1 | 1 | 2 | 41 |
| 10–11 | | 12 | 3 | 3 | 6 | 3 | 3 | 4 | 1 | 1 | | 4 | 1 | 1 | 42 |
| 11–12 | | 22 | 7 | 3 | 4 | 3 | 4 | 3 | 2 | 3 | 1 | 3 | 1 | 2 | 58 |
| p.m. | | | | | | | | | | | | | | | |
| 0–1 | | 26 | 5 | 3 | 1 | 4 | 3 | 3 | 1 | 6 | | | 1 | 1 | 54 |
| 1–2 | | 17 | 10 | 3 | 3 | 4 | 3 | 3 | 1 | 2 | 1 | 1 | | | 48 |
| 2–3 | 11 | 8 | 2 | 3 | 2 | 6 | 2 | 3 | 1 | 4 | 4 | 1 | 2 | | 38 |
| 3–4 | 10 | 9 | 2 | 5 | 4 | 2 | 3 | 2 | 2 | 3 | 1 | | 2 | 1 | 36 |
| 4–5 | 16 | 11 | 6 | 4 | 2 | 3 | 4 | 1 | | | 1 | 2 | 2 | | 36 |
| 5–6 | 3 | 11 | 4 | 2 | 4 | 2 | 4 | 2 | 4 | 4 | 3 | 1 | 2 | 4 | 47 |
| 6–7 | 15 | 15 | 5 | 7 | 7 | 2 | 3 | 2 | 3 | 2 | 1 | 2 | 1 | 1 | 51 |
| 7–8 | 13 | 16 | 12 | 11 | 5 | 9 | 6 | 5 | 2 | 5 | 1 | 1 | 1 | 3 | 77 |
| 8–9 | 9 | 12 | 11 | 6 | 3 | 4 | 5 | 3 | 4 | 3 | 3 | 3 | | 1 | 58 |
| 9–10 | 11 | 9 | 5 | 2 | 8 | 4 | 4 | 4 | 4 | 1 | 4 | 1 | 3 | 2 | 51 |
| 10–11 | 6 | 10 | 1 | 6 | 2 | 4 | 3 | 4 | 2 | 3 | 1 | 2 | 2 | 2 | 42 |
| 11–12 | 7 | 3 | 1 | 3 | | 2 | 2 | 4 | 2 | 5 | 3 | 5 | 2 | 3 | 35 |
| Sum | 101 | 318 | 173 | 126 | 99 | 92 | 81 | 78 | 53 | 67 | 45 | 42 | 43 | 40 | 1257 |

Before the great earthquake the seismic frequency at Gifu was normally less than 20 per month, so that the numerous individual shocks here recorded are part of the same seismic phenomenon, of which the disastrous earthquake was the most conspicuous feature. The relief of strain which is the real origin of the shock does not establish a state of complete equilibrium. A great fault slip, such for example as gave rise to the Assam earthquake of 1897 or the San Francisco earthquake of 1906, might over-rush the position of greatest stability, or it might not quite reach it. A succession of smaller slips would necessarily follow, giving rise to the so-called aftershocks. Strictly speaking, these aftershocks are as much part of the whole phenomenon as the great disastrous shock which made the phenomenon historic.

In many cases the large shock is worked up to by a series of preliminary but much smaller shocks. These are recognized as preliminary only after the great earthquake has come and gone. The dynamical problem is then one of a kind of which we have many illustrations in molecular physics. Because of the action of external forces a state of strain is induced in the material tending to a condition of instability. Yielding or rupture occurs and the fractured material takes up a less unstable grouping. Smaller yieldings follow until a fairly steady state of equilibrium is reached. This will continue for a greater or smaller interval of time until the forces work up a new state of instability. Another seismic disturbance follows, to be succeeded by a sufficient but still temporary state of stability; and so on at uncertain intervals of thirty to sixty years. The moderate earthquakes are no doubt subject to similar laws; but their aftershocks are too feeble to be distinguished from the ordinary small disturbances which are felt in seismic regions.

It is reasonable to suppose that many small yieldings to strain will occur near the seismic focus, but in such a way as not to produce any noticeable effect at the surface. What we have to deal with are the more intense changes which crop out at the surface as sounds or slight movements. Our records, even with the aid of instruments, can never be complete. We obtain but an indication of the life history of a complex of seismic disturbances which reach more or less abruptly their greatest intensity during a brief interval of a few minutes. Thereafter a rapid decay of frequency and intensity occurs, dwindling away with fluctuations through days, months, and years. Such decay is usually expressible mathematically by what is called an exponential function of the time; but Omori has shown by trial that it may in most cases be represented by a simple formula of the form

$$f=\frac{A}{B+t}$$

where $f$ is the average frequency reckoned over a certain

chosen interval, $t$ is the time from the occurrence of the earthquake, and $A$ and $B$ are constants to be determined for each case.

For example, by taking the half-daily frequencies from the table just given for Gifu and reckoning from the first half of October 29, Omori finds that the formula

$$f = \frac{440 \cdot 7}{t + 2 \cdot 31}$$

represents fairly well the half-daily frequencies, not only throughout the interval covered by the table, but during several succeeding years. Here $t$ is measured in half-days reckoning from 6 a.m. of October 29, and indicates the middle of the half-day over which $f$ is the frequency.

Omori has also shown that the relative intensities of earthquakes may be compared by the frequencies of their aftershocks.

The decay in frequency of aftershocks is always subject to fluctuations, and we have already seen that there is some evidence of a daily fluctuation having a maximum within a few hours of midnight. As regards annual fluctuations the nature of the statistics of aftershocks hardly allows any conclusion to be drawn. The number of years is much too limited; and the character of the month by month variation of frequency is largely determined by what happens in the early months immediately succeeding the great earthquake. We have seen above how the hourly statistics for the first few days stamp their character upon the whole body of statistics for thirteen days so as to bring into prominence a so-called eight-hours periodicity. A prominent disturbance or series of disturbances occurring in the early months after a great shock cannot fail to impress itself on the body of statistics of aftershocks; and any irregular series of numbers arranged in months and then in years will show maximum and minimum points. To ascribe to these more than a casual nature is, I am afraid, impossible in the circumstances.

By smoothing off the graphical representation of the time variation of aftershocks Omori finds evidence of certain

longer periods than months or years. To account for these fluctuations in terms either of tidal action of sun and moon or of barometric variations of pressure seems to be out of the question; and we are driven to accept these fluctuations as being a part of the character of the disturbance just as much as the great movement which works havoc is a part of it. Periodicity of an irregular kind there is; a periodicity which shows itself in the irregular waves of varying intensity which make up every strong or moderate shock, and which continues to show itself in the fluctuations of intensity as the seismic phenomenon works out its life history in a gradual decay. So far as present evidence goes these fluctuations are part of the phenomenon and depend very slightly if at all upon external influences such as might be referred to the action of sun or moon or atmospheric pressure.

# CHAPTER IX

## ELASTICITY

Elasticity of Bulk and Form. Rigidity and Incompressibility. Shear and Shearing Stress. Extension and Bending. Static and Kinetic Moduli. Elastic Constants for Rocks. Milne and Gray. Nagaoka and Kusakabe. Adams and Coker. Speed of Propagation of Disturbance.

WE have now to consider the manner in which seismic disturbances are transmitted through the material of the earth. This, as pointed out in chapter i, depends on the elasticity of matter.

The theory of elasticity is in the main concerned with the behaviour under stress of homogeneous isotropic substances ; and its application to movements in the intensely heterogeneous crust of our earth might at first seem to be altogether beside the mark. But the very irregularity of the heterogeneity may reasonably enough give to the earth's crust properties in the broad, more nearly resembling those of the homogeneous solid than of any definable crystalline substance. Moreover, the crust is a mere fraction of the earth ; and the earth as a whole may respond to elastic disturbances in a manner practically identical with the behaviour of an ideal sphere isotropic at each point, but varying from point to point in its physical and dynamical properties. Seismological studies have indeed indicated that this conception of the structure of the earth is sufficiently probable ; but even if it were not so the discussion of the elastic properties of such an ideal globe must tend to a greater intelligibility of what does occur in the real case.

Any application of elastic theory to the elucidation of earthquake phenomena is necessarily only approximate, and must not be supposed to mean that the earth is a homogeneous isotropic elastic solid. This was clearly recognized by Hopkins, who was the first to treat earthquake motion as illustrative of elastic vibrations.

There are two lines of discussion which are important. There is first the manner in which rock masses resist and yield to the combinations of stress brought to bear upon them. This might be called the statics of earthquakes. Then there is the transmission of the disturbances so produced through the mass ; evidently a kinetic problem.

When the form or bulk of a substance is in any way altered, the strain so produced in the vicinity of any point is associated with a definite stress whose components can be expressed in terms of the components of strain. The simplest case is that of an isotropic body which has been changed in bulk only. The associated stress is equal-valued all round, being a simple hydrostatic pressure when the volume is diminished, and an isotropic tension when there is expansion of volume. The ratio of the change of unit volume to the increase of pressure producing it is called the compressibility of the substance. In the case of liquids and solids the compressibility is a small quantity, for it requires a considerable stress to produce a measurable strain. It is more convenient for present purposes to follow Sir George Darwin's example and use the reciprocal of the compressibility, namely, the *incompressibility*, as the elastic constant. The larger the incompressibility the more nearly does the substance approach the ideal condition of a substance absolutely incompressible. For most practical purposes water may be treated as an incompressible fluid ; but when we come to consider the propagation of 'sound waves', that is, waves of compression and dilatation through it, we must treat it as a substance with a finite though small compressibility. All known substances possess compressibility, and therefore all can transmit disturbances involving change of volume or change of density. In other words, elasticity of bulk is a general property of matter, and the incompressibility is the modulus or multiplier by which we pass from the given strain (in this case compression) to the corresponding stress.

Fluids possess elasticity of bulk only ; for when any change of form is effected in the heart of a mass of fluid there is no tendency to recover the original form. It is

otherwise, however, with solids, which resist to a greater or less degree any effort to change their shape. Extension, flexure, and twist are the familiar modes in which solid bodies are distorted; and to these kinds of strain there correspond appropriate stresses, whose values can be estimated in terms of the strains by multiplication by a definite modulus. Now just as there is a strain involving pure change of volume without change of form, so there is a strain involving change of form unaccompanied by any change of bulk. The simplest type of such a strain is the case of a cube transformed by a sliding of layer over layer into a parallelepiped on the same base and of the same height as the cube. This is indicated in the figure. The arrow-headed lines represent the stresses, which by their action on certain faces produce and support the strain. Such a strain is called a simple shear, and the corresponding stress a shearing stress. A more symmetrical representation of a simple shear is indicated in the second figure, where a parallelepiped not quite cubical is sheared into a form which might be regarded as the image of the first form in a plane mirror. The figure shows only a section parallel to the plane of the shear. There is no change of area; but there is a lengthening along the one diagonal and a corresponding shortening along the other diagonal. This indeed

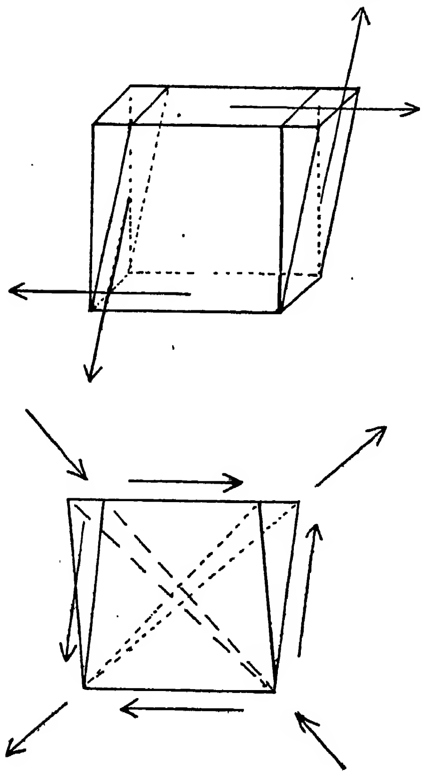


FIG. 29.

is another method of representing a shear, and the corresponding stress may be described as being composed of a pull or tension in the direction of lengthening, and an equal push or pressure in a direction at right angles. In this case the modulus by which we pass from the shear to the associated shearing stress is called the rigidity. The greater the rigidity the nearer does the substance approximate to the ideal case of an absolutely rigid solid.

The simple shear is practically realized when a cylindrical bar or wire is twisted about its axis of symmetry. Every small element originally cubical becomes sheared by an amount which is proportional to its distance from the axis.

Every strain, however, which involves different elongations in different directions necessarily involves a simple shear or change of form; and it can be shown that the most general strain can be built up of components which are either pure volume change or pure form change. When a bar is pulled out by a tension in one direction, it suffers contraction in all directions perpendicular to this direction of pull. The net result is a definite change of volume and a definite change of figure. Each of these changes is resisted by its appropriate elastic stress. But practically the important component of the strain is the extension, or change in unit length; and the modulus or multiplier by which we pass from this extension to the associated tension is known as Young's modulus. It is an elastic modulus of the same nature as the incompressibility and rigidity; but it is not a simple modulus. It is determined by an experiment in which volume and form both change. In the mathematical theory of the elasticity of isotropic solids Young's modulus is expressed in terms of the incompressibility $k$ and the rigidity $n$ by means of the formula $9nk/(3k+n)$. Now Young's modulus can be readily determined by experiment; and the rigidity also is easily measured by torsion experiments. Hence the incompressibility which is difficult of direct determination may be estimated from a knowledge of the other two.

In the case of simple flexure, as of a bent bow, the layers

on the outside of the bend must be lengthened, and those on the inside shortened; and as we pass from the one side of the bent bar or rod to the other, the strain passes through diminishing stages of elongation to zero and then increases through increasing stages of contraction. The associated stress is a tension in the elongated filaments and a pressure or longitudinal push in the contracted filaments. Thus the problem reduces to a particular case of strains involving Young's modulus. When a thin plank or rod is made to rest with its ends on two supports at the same horizontal level it will sag under action of its own weight; and a certain relation will exist between the length, the droop at the middle, the dimensions of the rod or plank, and the value of Young's modulus. Should the plank be not flexible enough to droop appreciably at the middle, it may be made to sag sufficiently by means of a load attached to the middle point, or by a pressure applied there; and again a certain relation will exist among the quantities already mentioned and the applied load or pressure. The experimental difficulty is to measure with sufficient accuracy the droop, which is necessarily a very small quantity. In really delicate work it is not possible indeed to measure it directly; and an indirect method based on optical principles is usually applied.

The method here indicated is particularly convenient for measuring Young's modulus for rocks. The earliest attempts to obtain this and other elastic constants in connexion with seismological phenomena were made by Milne and Gray in Japan in 1881. Much more recently, Professor Nagaoka of Tokyo has investigated the elastic constants of a great many rocks of various geological ages; and his work has been greatly extended and developed by Kusakabe. A further important contribution to the subject has been made by Professors F. D. Adams and E. G. Coker,[1] both at the time of the research members of the staff of McGill University, Montreal. In their method of experiment

[1] *An Investigation into the Elastic Constants of Rocks, more especially with Reference to Cubic Compressibility.* Carnegie Institution, Washington, 1906.

Young's modulus was measured directly for compressing stress by means of a delicate instrument designed by Ewing; and the lateral expansion accompanying the longitudinal compression was measured by means of a special apparatus designed by Coker[1] and used already by him in his investigations on the elastic properties of metals. The ratio of the lateral elongation to the longitudinal compression under longitudinal pressure, or the ratio of the lateral contraction to the longitudinal elongation under longitudinal tension, is known as Poisson's ratio. In the elastic theory of isotropic solids this ratio is equal to $(3k-2n)/(6k+2n)$, where $k$ and $n$ are the incompressibility and rigidity respectively. The experiment gives Young's modulus and Poisson's ratio, from which the incompressibility and rigidity may be at once calculated.

It should be noted that the methods just described for measuring the various elastic constants of a solid substance assume the ordinary simple theory for an isotropic substance. But it is obvious that the vast majority of rocks cannot be regarded as even approximately isotropic. This fact must be kept in mind when we are applying any of the results to the elucidation of seismic phenomena.

Thus Nagaoka and Kusakabe measure the rigidity directly by torsion experiments; but the value of Young's modulus is deduced from flexure experiments by use of a formula which is true only for isotropic solids strained within the limits of perfect elasticity. On the other hand, Adams and Coker measure Young's modulus directly; but the rigidity and incompressibility are got at indirectly by calculation from Young's modulus and the measured value of Poisson's ratio.

In the specimens of marbles, granites, and basic intrusives experimented on by Adams and Coker, the value of Poisson's ratio varies from 0·1977 for a certain specimen of granite to 0·29 for Ohio sandstone. This means, in accordance with the usual theory, that Young's modulus varies from 2·4 to 2·6 times the rigidity and from 1·8 to 1·2 times the incompressibility.

[1] *Proceedings of the Royal Society of Edinburgh*, vol. xxv, p. 452, 1904.

The relation between Young's modulus and the rigidity is, according to the isotropic theory,

$$E/2n-1=\text{Poisson's ratio},$$

which shows that $E$ cannot reasonably be less than $2n$ or greater than $3n$. For in the former case we should have a negative value for Poisson's ratio, that is, a lateral contraction as well as a longitudinal contraction under a longitudinal compressing stress; and in the latter case we should have the lateral *areal* expansion greater than the longitudinal contraction, so that, under the same kind of compressing stress, there would be increase of volume.

But if we turn to Nagaoka's original list we shall find many examples of rocks for which this ratio of Young's modulus to the rigidity is 4, 5, 6, or even 7. This simply means that the materials are not even approximately isotropic. Indeed, nearly all the rocks which are so characterized are shales, slates, and schists, the aeolotropy of which is apparent at a glance. To such rocks accordingly the method of measuring Young's modulus by flexure is not strictly applicable. What the experiments give is the so-called flexural rigidity, which is not a fundamental elastic constant.

The following table shows how many of the rocks investigated by Nagaoka lie distinctly outside the limits assigned above for reasonable values of Poisson's ratio. They are grouped also according to geological age. There are three columns containing the number of rocks for which the ratio $E/n$ is less than 2, between 2 and 3, and greater than 3.

| Geological Age | Number of Specimens | | | |
|---|---|---|---|---|
| | $2n>E$ | $2n<E<3n$ | $E>3n$ | Total |
| Archaean | 0 | 4 | 2 | 6 |
| Palaeozoic | 3 | 14 | 16 | 33 |
| Mesozoic | 0 | 3 | 5 | 8 |
| Cainozoic | 8 | 31 | 6 | 45 |

It is curious to note the marked difference between the Palaeozoic and Cainozoic groups of rocks in regard to the characteristic under discussion. In the older rocks we find

proportionately greater deviations from the conditions of isotropy.

Nagaoka's results bring out very markedly the fact that the average rigidity in the rocks of a given age is greater the older the age. This is shown in the following table of the range of rigidities and the average rigidity of the sets of rocks investigated. The unit is $10^{10}$ C.G.S. units.

| Geological Age | Number of Specimens | Range of Rigidities | Average Rigidity |
|---|---|---|---|
| Archaean | 6 | 16 to 31·6 | 22·3 |
| Palaeozoic | 33 | 4·3 to 31 | 14·8 |
| Mesozoic | 8 | 2·4 to 23·2 | 12·7 |
| Cainozoic | 45 | 1·0 to 18·5 | 6·3 |

Clearly the effect of prolonged pressure and the metamorphic action of the internal heat of the earth are such as to increase the resistance to distortion of the materials forming the earth's crust. There is at the same time a slight increase in the average density of the material with age, but the change in density is not nearly so marked as the change in rigidity. There is thus a distinct tendency for the ratio of the rigidity to the density to increase with the geological age of the rock, so that an elastic disturbance will be propagated with a higher speed through the more deeply seated rock.

The well-marked imperfect elasticity of many of these rocks, especially of the sandstones, is a feature which seriously interferes with the determination of the rigidity. Nagaoka gives an illustration of the gradual way in which a block of sandstone yielded to a comparatively small twisting stress, so that after the lapse of twenty minutes the accompanying strain had increased from its original value by nearly 30 per cent. of that value. In such a case there is incomplete recovery when the stress is removed; and the difficulty is to get a true measure of the rigidity. It has, as Kusakabe expresses it, a doubly indefinite character. It depends upon the previous history of the material, and especially upon the manner of approach to the final stress and connected strain which are the immediate

object of inquiry. The phenomenon is well known to all who have experimented on the elasticity of metals ; but it is particularly evident in the case of sandstone rocks (see Fig. 1, p. 15). From our present point of view, namely, the propagation of elastic disturbances through the material of the earth, the rigidity for vanishingly small strains is what is wanted. Now all the experiments show that the smaller the strain the higher the rigidity for any particular specimen, a fact which is in accordance with the theory of viscosity now generally accepted. I content myself with giving the highest values of the rigidity obtained by Kusakabe in different kinds of rocks. Steel, copper, and flint glass are added for comparison.

| Age | Substance | Density | Rigidity | Speed of Propagation |
|---|---|---|---|---|
| Archaean | Serpentine | 2·71 | 52·2 | 4·4 |
| | Quartz-schist | 2·64 | 28·9 | 3·3 |
| Palaeozoic | Pyroxenite | 2·9 | 49 | 4·33 |
| | Granite | 2·54 | 16·9 | 2·58 |
| | Marble | 2·64 | 9·15 | 1·85 |
| Tertiary | Rhyolite | 2·36 | 3·12 | 1·16 |
| | Sandstone | 2·64 | 2·20 | 1·09 |
| | Andesite | 2·63 | 8·09 | 1·75 |
| | Steel | 7·85 | 82 | 3·23 |
| | Copper | 8·84 | 45 | 2·26 |
| | Flint Glass | 2·94 | 24 | 2·86 |

The gradual yielding of the substance to a steady moderate stress is not so marked in flexure experiments as in torsion experiments. This is to be expected inasmuch as, under flexure, part of the rod is in compression, and once the limits of practically perfect elasticity are exceeded the material will yield more readily to an extending force than to a compressing force. From this point of view also Adams and Coker's method of experimenting with compressing stresses is preferable to the more usual and somewhat simpler method of applying extending loads.

Adams and Coker selected their rocks with great care, having regard to uniform and approximately isotropic structure as well as to freedom from all flaws and cracks in the test pieces.

I reproduce the results in full, with the added information of the geological ages and the densities of the rocks used.[1]

| Rock | Age | Young's Modulus | Poisson's Ratio | Rigidity | Incompressibility | Density |
|---|---|---|---|---|---|---|
| Black Belgian Marble . . | Palae. | 76·4 | 0·2780 | 29·82 | 57·26 | 2·7 |
| Carrara marble . . . . | Meso. | 55·4 | ·2744 | 21·71 | 40·90 | 2·72 |
| Vermont marble . . . | Palae. | 52·4 | ·2630 | 20·69 | 36·80 | 2·71 |
| Tennessee marble . . . | Palae. | 62·1 | ·2513 | 24·82 | 41·15 | 2·70 |
| Montreal Limestone . . | Palae. | 63·5 | ·2522 | 25·04 | 42·50 | 2·69 |
| Baveno granite . . . . | | 47·1 | ·2528 | 18·75 | 31·79 | 2·61 |
| Peterhead granite . . . | Palae. | 57·1 | ·2112 | 23·40 | 33·00 | 2·63 |
| Lily Lake granite . . . | Palae. | 56·3 | 1·1982 | 23·30 | 31·03 | 2·63 |
| Westerly granite . . . | Palae. | 50·9 | ·2195 | 20·80 | 30·29 | 2·63 |
| Quincy granite (1) . . . | Palae. | 46·4 | ·2152 | 19·16 | 27·50 | |
| „ „ (2) . . . | | 56·8 | ·1977 | 23·73 | 31·40 | |
| Stanstead granite . . . | Palae. | 39·2 | ·2585 | 15·56 | 27·18 | 2·68 |
| Nepheline syenite . . . | Palae. | 62·9 | ·2560 | 25·05 | 42·90 | 2·62 |
| New Glasgow anorthosite | Arch. | 82·5 | ·2620 | 32·75 | 57·60 | 2·72 |
| Mount Johnson essexite . | Palae. | 67·1 | ·2583 | 26·70 | 46·50 | 2·82 |
| New Glasgow gabbro . . | Arch. | 108·0 | ·2192 | 43·80 | 65·89 | |
| Sudbury diabase . . . | Arch. | 94·9 | ·2840 | 37·00 | 73·29 | 3 |
| Ohio Sandstone . . . | Palae. | 15·8 | ·2900 | 6·12 | 12·50 | |

From these values we can calculate the speeds of propagation of the various types of wave according to the formula given below. The New Glasgow gabbro (density 3, assumed) gives the highest distortional speed, namely, 3·8 km. per sec.; and the compressional dilatational speed is 6·4 km. per sec. The latter type of disturbance travels slightly faster in the Sudbury diabase. Here also there is a tendency for the elastic constants to increase with the age of the rock.

In all these cases it is the static modulus which is being measured; but in the propagation of elastic disturbances through the material it is the kinetic modulus which comes into play. In the case of metal wires, the kinetic and static values of a particular modulus differ slightly; and, according to Kusakabe's ingenious experiments, the difference between the kinetic and static values of the flexural rigidity in rocks is in many cases of considerable magnitude. His method was to measure the period of natural vibration of a short rod or prism of the rock, the one end of which was

[1] Dr. Horne of the Geological Survey of Scotland furnished me with the geological ages of the rocks; and these were checked by Professor Adams when he supplied me with the densities.

firmly clamped. The free end was kept in vibration like a tuning fork by taps of a hammer worked by an electro-magnet. To this free end was attached a fine copper wire, which was led over a bridge and kept in a state of suitable tension by an appended weight. The vibrating prism of rock imposed a forced vibration upon the wire, the length of which was carefully adjusted until the amplitude of vibration attained a maximum. This happened when the vibration period of the rock prism was one of the natural periods of vibration of the stretched copper wire. Since this could be calculated in terms of the length and tension of the wire, the period of vibration of the rock became known; and application of the usual theory gave the flexural rigidity and the kinetic Young's modulus. Bearing in mind the imperfect elasticity of rock as evidenced by the yielding during application of stress, we should expect the static modulus to be less than the kinetic modulus, since in the latter case the strains are smaller and there is no opportunity for the time lag to declare itself. This was what Kusakabe found to be generally the case; but there were a good many instances in which the kinetic modulus was the smaller. This is probably to be referred to the different condition of the material in the two experiments. Kusakabe himself showed that the presence of moisture in the specimen had a great effect upon the value of the flexural rigidity, making the material much less resistant than in the dry state. As pointed out by Nagaoka, this suggests that superficial earthquakes may be more likely to follow times of heavy rains, which, by soaking through the rocky crust, will render it more liable to yield to stresses acting on it. It is interesting to note in this connexion that the inhabitants of Comrie in Perthshire, some fifty years ago when shocks were frequent in that locality, believed that earthquakes were more frequent after than before excessive rainfall.

Some of the most important applications to be made have to do with the propagation of elastic disturbances through the material of the earth. The speed of propagation of a disturbance through an elastic substance depends upon the density of the substance and the appropriate elastic

modulus. In every case the square of the speed of propagation is measured by the ratio of the modulus to the density, or

$$V^2 = E/D$$

where $E$ represents the modulus and $D$ the density or mass in unit volume.

In the case of fluids there is only one kind of elastic modulus, namely, the incompressibility; and the elastic disturbance must be a change of density with a corresponding change of pressure. The most familiar example is the transmission of sound through air, by means of a succession of condensations and rarefactions. This condensational rarefactional disturbance is audible as sound, because the pressure and density vary at a sufficiently rapid rate. Condensations and rarefactions may follow one another at too slow a rate to be recognizable as sound; but the air is none the less transmitting elastic vibrations, although our sense of hearing is unable to detect them. Not until the successive maxima and minima of pressure follow one another at a rate of thirty or forty times a second does the passing disturbance appeal to our sense of hearing. The bearing of this on earthquake sounds has been discussed in chapter ii.

When the successive disturbances follow one another at regular intervals we speak of them as forming a train of waves of a definite period and wave-length. The period is the time taken by any small portion of the medium to go through all its phases of configuration and come back to the original state of pressure and volume. During the lapse of time equal to one period the disturbance advances a wave-length. Hence the speed of propagation is equal to the wave-length divided by the associated periodic time. In rapid vibratory motion this periodic time is so short that it is often more convenient to speak of the number of vibrations or pulses per second. This number is called the frequency; its value is numerically equal to the reciprocal of the periodic time. Hence we find that the speed of propagation is equal to the product of the wave-length and the frequency, a relation which enables us to calcu-

late the wave-length when the other two quantities are known.

When we pass to the consideration of elastic solids we see that different kinds of elastic disturbances may be propagated corresponding to the different kinds of elasticity involved. Imagine, for example, a long straight wire with the one end fixed, and let a twist be given to the free end. This twist will travel to and fro along the wire with a speed which is measured by the square root of the ratio of the kinetic rigidity to the density. Again, if a blow be struck on the end in the direction of the rod or wire, a longitudinal disturbance will be started which will run to and fro along the rod with a speed equal to the square root of the ratio of the kinetic Young's modulus to the density. A wooden rod, tightly clamped at the lower end, may be made to emit a musical note of a definite pitch by gently drawing over its surface with a steady pressure a piece of resined leather. The friction sets up longitudinal vibrations which pass to and fro along the rod like the air pulse along a closed organ pipe.

The speed of propagation of elastic disturbances through extended masses does not, however, depend on Young's modulus either directly or indirectly. The speed of propagation of what is known as the condensational wave depends upon a modulus which is equal to the sum of the incompressibility and $\frac{4}{3}$ of the rigidity—in symbols, $k+4n/3$. Within the limits assigned above for the value of Young's modulus relatively to the rigidity, this wave modulus is always greater than Young's modulus. It is also obviously greater than the rigidity. Hence the so-called condensational wave in elastic solids is propagated with the greatest speed of all the possible waves which have been referred to. It is well to note that this disturbance of most rapid transmission involves change of shape as well as change of volume, so that both the fundamental moduli enter into the expression.

In addition to this condensational disturbance, there may be a purely distortional disturbance propagated through the extended mass with a speed which depends only on the

rigidity. The displacements of the medium which accompany the passing of this type of wave take place at right angles to the direction in which the wave is being propagated. It is often referred to as the transverse wave, because of the vibratory motions being transverse to the direction of propagation, just as in the case of a tightly stretched string. Similarly the wave of mixed modulus, $k + 4n/3$, is sometimes spoken of as the longitudinal wave.

The surface undulations which are experienced near the epicentre of a strong earthquake imply flexural strains in the surface layers, and the speed of propagation of these waves may depend partly on the flexural rigidity and partly on gravity. They can hardly be considered as purely elastic, and are no doubt powerfully influenced by the viscosity of the material. Away from the immediate region of the earthquake origin, and well below the surface, the vibrations which are transmitted are certainly not flexural; and under the necessary constraints at great depths they are less influenced by viscosity.

# CHAPTER X

## ELASTIC WAVES

Reflexion and Refraction of Waves. Two Flexible Ropes of Different Weight. Two Elastic Media with a Common Boundary. One type of Wave Motion in Fluids, Condensational. Two Types of Wave Motion in Solids, Condensational and Distortional. Reflexion and Refraction of Elastic Waves and Boundary of two Media. Rock and Water. Rock and Rock. Rock and Air. Water and Air. Earthquake Barriers. Seismic Energy largely retained in Crust. Interference Phenomena. Viscosity. Wave Groups.

Let us imagine a disturbance of a complex character originating some ten or twenty miles below the earth's surface. This will generate in the surrounding material two distinct types of wave motion propagated with different velocities. Each type will suffer reflexions and refractions at surfaces separating regions of different physical properties ; and will also experience reflexion wherever it impinges internally on the surface of the earth. The final result will be an excessively complex motion at every point of the shaken area.

The dynamical process by which waves are propagated and by which they pass from one medium to another—for the process is one and the same—may be illustrated by experiments with ropes or flexible cords not too tightly stretched.

For example, fix the one end of an ordinary clothes' rope to a point eight feet or more above the floor, and hold the other end in the hand so that the rope hangs freely in air. If now the rope be drawn down by the other hand and then let go a disturbance will be observed passing along the rope to the point of fixture in the wall, and then back again after reflexion. The hand which holds the rope will feel a succession of tugs as the disturbance comes back after successive reflexions at the far end.

The essential phenomena are studied to greater advantage

by use of a flexible cord heavier than the ordinary rope. This is conveniently done by filling a long piece of rubber tubing with small shot. With such an arrangement the disturbance passes along at a comparatively slow rate, and the instants of its reflexion at the two ends can be timed with ease. Let a second piece of tubing of narrower bore be similarly filled. Then, when the two are set side by side and simultaneous disturbances are sent along them it will be seen that the disturbance travels more quickly along the thinner tube, that is, the tube weighing less per foot. Now let the two tubes be tied together, end to end, at $C$, so as to form a single stretch, the other extremity $B$ of the heavier tube being fixed to the wall, while the free extremity $A$ of the lighter tube is held in the hand. By plucking the tube near $A$ we start a disturbance which

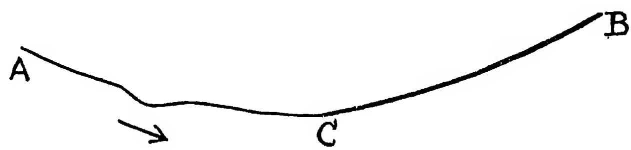



FIG. 30.

travels along towards $C$ with the speed belonging to the thinner tube. On reaching $C$ the disturbance separates into two parts, the one part passing back by reflexion towards $A$, and the other part passing on with a slower velocity of propagation towards $B$. Here it is reflected and passes back towards $C$, where again it breaks up into two parts, one of which continues towards $A$, while the other is reflected towards $B$. Meanwhile the first reflected part in the thinner tube suffers reflexion at the hand, and is sent towards $C$ again, once more to be split up into two.

Now if instead of a 'point boundary' between two flexible strings of different weight and tension, we think of a 'surface boundary' between two extended masses of different density and elasticity, we pass to the case of the propagation of elastic waves through solids. Let us suppose for simplicity that the interface between the two elastic media is a plane, and that the disturbance travelling in the

one medium is of such a nature that it actuates every point of the interface with exactly the same kind of motion at the same instant. Such a disturbance is called a plane wave with its wave front parallel and its direction of propagation perpendicular to the interface. This wave is falling normally on the interface; and the wave started in the other medium will pass on with different speed, but still in the same direction as the original wave of disturbance. At the same time, just as in the case of the two flexible tubes, a 'reflected' wave will be sent back into the first medium.

When, however, the disturbance falls obliquely on the interface so that different parts of the interface are affected at different times, the problem becomes much more complicated. The plane wave sweeps along the interface in a manner similar to the way in which a long roller is often seen to sweep along the side of a breakwater, each part of the breakwater receiving the dash of the wave at a different instant of time. From each successively disturbed point of the interface disturbances are passed on into the second medium and also back into the first medium; and these disturbances will combine to form resultant plane waves propagated in the respective media. The manner in which a series of disturbances combine to form well-marked wave fronts may be illustrated by dropping into a stretch of still water a metal rod inclined to the horizontal. The disturbance begins where the lower end of the rod first touches the water; and when the whole rod has become immersed, the disturbed region of the water will be seen to be bounded by two wavelets forming a wedge, which gradually increases in size as the wavelets pass outwards in each direction.

When the two media are elastic solids a special complexity comes in because of the two types of waves referred to in last chapter. Even if the original disturbance in the first medium be of one type only it will, except under very special conditions, generate at every boundary where reflexion and refraction occur not only reflected and refracted waves of its own type, but also those of the other type.

This is at once seen to be necessary when we picture to ourselves the nature of the motion accompanying the transmission of the two types of wave. When the plane wave is of the distortional type the vibrations of the particles of the medium take place in the wave front, that is, perpendicular to the direction of propagation of the wave. In the condensational type of wave the vibration of the medium has a component perpendicular to the wave front, and in the case of plane waves passing through an extended solid the vibration is wholly in this direction normal to the wave front, that is, parallel to the direction of propagation of the wave.

In the case of oblique incidence, the longitudinal vibration in the incident wave will produce a disturbance in the boundary which cannot possibly be normal to the wave front of the reflected wave. There will be a component in the wave front of this reflected wave, and consequently a distortional transverse wave will be started with a transverse vibration. Hence, in general, an obliquely incident dilatational wave will give rise to a distortional as well as a dilatational reflected wave in the first medium, and also two refracted waves, one distortional and the other dilatational, in the second medium. Similarly a purely distortional wave incident obliquely on the boundary will generate four waves altogether, two refracted and two reflected.

The various angles of reflexion and refraction are easily calculated in terms of the angle of incidence, it being noted that the surface trace is common to all the waves. The speed of propagation of each wave is, so to speak, the component in its direction of the speed of propagation of the surface trace.

For example,[1] let a condensational wave be incident at an angle $\theta$ to the boundary surface separating two media ;

[1] The results which follow, as well as much of what precedes, are taken from my paper on the *Reflexion and Refraction of Elastic Waves with Seismological Applications* (*Phil. Mag.* for July 1899), which paper was a reprint with additions of a previous elastic discussion of *Earthquakes and Earthquake Sounds* (*Trans. Seism. Soc. of Japan*, vol. xii, 1888).

and let $v$, $v'$ be the speeds of propagation of the condensational waves in these two media, and $u$, $u'$ the speeds of propagation of the distortional waves. The corresponding angle $\theta'$ for the refracted condensational wave, and the angles $\phi$, $\phi'$ for the reflected and refracted distortional waves, are given by the equations

$$v/\sin\theta = v'/\sin\theta' = u/\sin\phi = u'/\sin\phi'.$$

Now, as $u$ is less than $v$, there will always be a reflected distortional wave, except at normal incidence when $\theta = 0°$, and at grazing incidence when $\theta = 90°$. There will be refracted waves for all except the limiting incidences, if $v$ is greater than $v'$. If $v$ should be intermediate in value to $v'$ and $u'$, there will be no refracted condensational wave for angles of incidence higher than a certain critical angle; the condensational wave will be totally reflected. If $v$ should be less than both $v'$ and $u'$, there will be special angles of total reflexion for both kinds of waves. When the critical value corresponding to the refracted distortional wave is reached there will be total reflexion of both types of wave, and the whole energy of the incident wave will be divided between the two reflected waves.

If the incident wave is a distortional wave, there must always be a critical angle of incidence for and above which the reflected condensational wave vanishes. The existence of such critical angles for the refracted waves will depend upon the relative values of $u$, $v'$, $u'$—the condition for the possibility of total reflexion being that $v$ is less than $v'$.

If one of the media is a fluid, there can be no distortional wave in it. This therefore forms a distinctly simpler case than when the media are both elastic solids, and a somewhat detailed account of a special case will be of service in indicating the manner in which a wave breaks up at the boundary of two substances.

Let us take water and rock[1] as the two media, the

[1] The elastic constants here used were selected in 1888 on the basis of Gray and Milne's experiments. A comparison with the table on page 165 shows that they represent an inferior type of granite.

constants being assumed to be as in the following table :—

| | Water | Rock |
|---|---|---|
| Density . . . . . . . | 1 | 3 |
| Incompressibility . . . | $2{\cdot}2 \times 10^{10}$ | $25 \times 10^{10}$ |
| Rigidity . . . . . . . | | $15 \times 10^{10}$ |
| Speed of $v$ wave . . . | 0·883 | 2·39 |
| Speed of $u$ wave . . . | | 1·38 |

The following tabulated results have been worked out for the case of a disturbance passing from rock to water. The quantities tabulated are the energies in the various types of wave, that of the incident wave being taken as unity. In the first table the incident wave in the rock is condensational; and since this kind of wave in the rock has a higher speed of propagation than either of the other two, there are no critical angles of total reflexion. In the second table the incident wave is distortional; and as its speed of propagation is less than that of the condensational wave in the rock, there is a critical angle of incidence for and above which there is no reflected condensational wave.

The quantities $A$, $A_1$, $A'$ represent the energies of the incident, reflected, and refracted condensational waves; $B$, $B_1$, $B'$ the energies of the similar set of distortional waves. The corresponding angles of incidence, reflexion, and refraction are given in contiguous columns—$\theta$ referring to the condensational and $\phi$ to the distortional waves.

INCIDENT WAVE CONDENSATIONAL.

| Incident | | Reflected | Refracted | | Reflected | |
|---|---|---|---|---|---|---|
| $\theta$ | A | $A_1$ | $\theta$ | A′ | $\phi_1$ | $B_1$ |
| 0° | 1 | ·599 | 0° | ·401 | 0° | 0 |
| 10° | 1 | ·536 | 3° 49′ | ·397 | 5° 45′ | ·071 |
| 20° | 1 | ·377 | 7° 32′ | ·370 | 11° 23′ | ·254 |
| 30° | 1 | ·195 | 11° 2′ | ·333 | 16° 35′ | ·456 |
| 40° | 1 | ·056 | 14° 15′ | ·293 | 21° 47′ | ·660 |
| 50° | 1 | ·006 | 17° 4′ | ·244 | 26° 15′ | ·753 |
| 60° | 1 | ·014 | 19° 22′ | ·206 | 30° | ·775 |
| 70° | 1 | ·031 | 21° 5′ | ·188 | 32° 15′ | ·783 |
| 80° | 1 | ·000 | 22° 9′ | ·182 | 34° 39′ | ·818 |
| 89° | 1 | ·616 | 22° 31′ | ·069 | 35° 16′ | ·314 |
| 90° | 1 | 1 | | 0 | | 0 |

INCIDENT WAVE DISTORTIONAL

| Incident | | Reflected | Reflected | | Refracted | |
|---|---|---|---|---|---|---|
| $\theta$ | B | $B_1$ | $\theta_1$ | $A_1$ | $\theta'$ | $A'$ |
| 0° | 1 | ·1 | 0° | ·000 | 0° | ·000 |
| 10° 23′ | 1 | ·711 | 20° | ·253 | 7° 32′ | ·036 |
| 21° 47′ | 1 | ·222 | 40° | ·656 | 14° 15′ | ·126 |
| 30° | 1 | ·014 | 60° | ·779 | 19° 22′ | ·206 |
| 34° 39′ | 1 | ·027 | 80° | ·815 | 22° 9′ | ·157 |
| 35° 16′ | 1 | ·679 | 89° | ·311 | 22° 31′ | ·007 |
| 35° 6′ | 1 | 1 | 90° | ·000 | 22° 31′ | ·000 |
| 36° | 1 | ·584 | | | 22° 56′ | ·415 |
| 40° | 1 | ·461 | | | 25° 14′ | ·539 |
| 50° | 1 | ·504 | | | 30° 33′ | ·495 |
| 60° | 1 | ·506 | Non-existent | | 35° 4′ | ·494 |
| 70° | 1 | ·520 | | | 38° 34′ | ·480 |
| 80° | 1 | ·634 | | | 40° 47′ | ·366 |
| 89° 45′ | 1 | ·818 | | | 41° 34′ | ·183 |
| 90° | 1 | 1 | | | | ·000 |

In the first table $B$ and $B'$ of course do not appear, and in the second table $A$ and $B'$ do not appear.

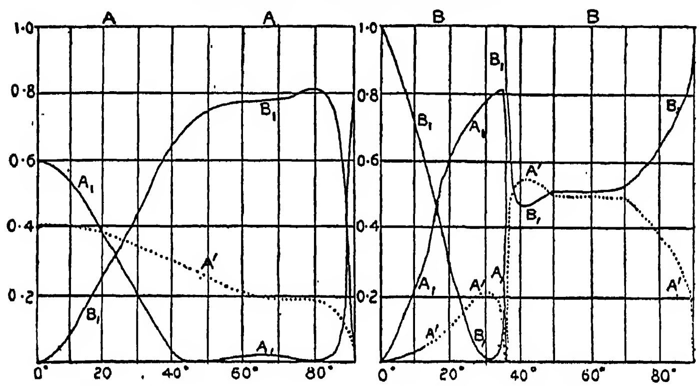
It should be mentioned that each wave-energy is calculated independently; and a test of the accuracy of the calculations is afforded by the condition that the energy of the incident wave must be fully accounted for. In other words, since in every case the incident energy (either $A$ or $B$) is taken as unity, the sum of all the others must be unity.

The chief peculiarities embodied in these tables are shown graphically in the corresponding curves (Fig. 31). Any one curve represents the manner in which the energy of each wave depends on the angle of incidence. The angles of incidence are measured off along the horizontal line; and the corresponding energies are represented by the ordinates perpendicular thereto. The energy of the incident wave is represented by the straight line at unit distance from the line along which the angles of incidence are measured off, i. e. the top horizontal line.

The first set of curves shows the state of things for an incident condensational wave. For the sake of brevity, we shall occasionally refer to the different waves by the letters $A$, $A_1$, $A'$, $B$, $B_1$, chosen to represent their energies. At perpendicular incidence condensational waves only are started at the bounding surface; and as the angle of incidence increases

the energies of both of these diminish. $A'$, the energy of the wave in the water, seems to fall off continuously until it vanishes at grazing incidence. The $A$-wave, however, vanishes at two distinct incidences, and after 80° is reached begins to increase till at 90° it attains unity. The behaviour of this reflected condensational wave is extremely curious, the wave being practically non-existent for incidences between 50° and 80°. The greater part of the energy of the incident wave is then accounted for by the $B_1$ or reflected

ROCK AND WATER INTERFACE




Condensational Wave incident     Distortional Wave incident

FIG. 31.

distortional wave. For incidences higher than 45° three-quarters of the whole incident energy is so transformed. It will be noticed that up to pretty high angles of incidence the energy transmitted to the water does not suffer any very great falling off.

Turning now to the second set of curves, which show the state of things for an incident distortional wave, we meet with some very curious relations. For reasons already discussed, the $A_1$-wave cannot exist for incidences higher than a certain critical value, which depends on the rock itself. The energy of this wave, however, attains a considerable maximum value for an angle of incidence slightly below this critical value. Almost for the same incidence, the energy of the $B_1$-wave falls to a very low minimum,

almost vanishing indeed. Comparing this first portion of the second set of curves with the first set of curves as a whole, we see a general resemblance between the two. That is, the energy of the reflected wave of the same type as the incident wave rapidly falls off to a minimum as the angle of incidence grows, while that of the reflected wave of the other type rapidly increases to a maximum. Finally the energy of the reflected wave of the same type, in both cases quite abruptly, runs up to equality with the incident wave. In the second set of curves this happens at the angle of total reflexion; for, not only does the $A_1$-wave vanish, but so also does the $A'$-wave—which indeed never attains any great significance at the lower incidences. After the critical angle of incidence is passed, however, the energy of the $A'$-wave soon reaches a maximum, being then of greater value than that of the $B_1$-wave, and gradually falls away to zero, while the energy of the $B_1$-wave as gradually rises to unity.

A glance at the two sets of curves shows that the incident distortional wave is, at the higher incidences, much more efficient than the condensational wave in creating a progressive disturbance in the water. The angle of refraction can never exceed 42°; so that even for very high incidences the wave in the water will travel upwards to the surface fairly directly. Here I think we may have the explanation of the curious bumpings which have sometimes been felt at sea (chapter vi). Sounds will be heard if the periodic time of any of the components in the wave-motion is short enough, and if at the same time the intensity is sufficient to give rise to *audible* sound waves in the air, either directly or indirectly through the medium of such a solid as a ship. According to Colladon's experiments at the Lake of Geneva, the speed of sound in water at 8°·1 C. is 1435 metres per second. This gives 14·35 metres (or about 8 fathoms) for the wave-length of a wave whose pitch is 100 vibrations per second. A slower vibration will of course give a longer wave-length; and a quicker a shorter. But enough has been said to show that in such a wave of condensation we have something quite fitted to affect even a large ship as a whole.

All that has been said regarding the transference of

vibrations from rock to water will, in a general way, hold true of their transference from rock to air. The point of special interest is that whatever be the angle of incidence in the rock the refracted wave will pass into the air in a direction which is very slightly inclined to the normal. Thus the incompressibility of air being $1{\cdot}41 \times 10^6$, and the density 0·0013, we find for the speed of propagation the value 0·329 kilometres per second, or 0·204 miles per second. With the same values as formerly for the rock constants we find

$$\sin \theta' = 0{\cdot}085 \sin \theta = 0{\cdot}147 \sin \phi.$$

If $\theta = 90°$, $\theta'$ will be 4° 54′, so that with the condensational wave incident in rock at nearly grazing incidence the refracted condensational wave in air will pass off in a direction only 5° removed from the direction of the normal. Similarly for the distortional wave in rock incident at grazing incidence ($\phi = 90°$) the resulting refracted condensational wave in air will make an angle of only 8° 30′ with the normal to the surface. At whatever incidences the various disturbances in the rock impinge upon the surface, the disturbance transmitted into the air will pass off in directions which will all be included within a small cone with axis along the normal and of semi-vertical angle less than 9°.

The following detailed calculation is made for the case of a rock whose elastic constants are as above, but whose density is 2000 times that of air. The speeds of propagation will be less than the former values in the ratio of 0·931 to 1.

Distortional Wave incident in Rock

| Incident | | Reflected | | | Refracted | |
|---|---|---|---|---|---|---|
| $\phi$ | B | $B_1$ | $\theta$ | $A_1$ | $\theta'$ | $A'$ |
| 0 | 1 | 1 | | 0 | | 0 |
| 14° 2′ | 1 | ·534 | 25° | ·466 | 1°·6 | ·00002 |
| 26° 34′ | 1 | ·025 | 51° | ·975 | 3° | ·00006 |
| 33° 40′ | 1 | ·003 | 74° | ·997 | 3°·7 | ·00006 |
| 35° 13′ | 1 | 1 | 90° | 0 | 3°·8 | 0 |
| 39° 48′ | 1 | 1 | imaginary | | 4°·3 | ·00019 |
| 45° | 1 | 1 | ,, | | 4°·7 | ·00016 |
| 59° 2′ | 1 | 1 | ,, | | 5°·7 | ·00014 |
| 73° 18′ | 1 | 1 | ,, | | 6°·3 | ·00014 |
| 84° 17′ | 1 | 1 | ,, | | 6°·6 | ·00006 |

CONDENSATIONAL WAVE INCIDENT IN ROCK

| Incident | | Reflected | Refracted | | Reflected | |
|---|---|---|---|---|---|---|
| $\theta$ | A | $A_1$ | $\theta'$ | $A'_1$ | $\phi'$ | $B_1$ |
| 0 | 1 | 1 | | ·00013 | | 0 |
| 14° 2′ | 1 | ·828 | 0°·9 | ·00013 | 8° | ·172 |
| 26° 34′ | 1 | ·464 | 1°·7 | ·00011 | 15° | ·536 |
| 45° | 1 | ·079 | 2°·7 | ·00009 | 24° | ·921 |
| 59° 2′ | 1 | ·0002 | 3°·3 | ·00007 | 30° | ·9997 |
| 73° 18′ | 1 | ·003 | 3°·7 | ·00006 | 34° | ·997 |
| 84° 17′ | 1 | ·091 | 3°·8 | ·00005 | 35° | ·909 |

The energy which escapes into the air is so small that practically the whole energy remains in the rock. The general behaviour of the phenomenon is very similar to what was found in the case of rock and water, the differences being differences of degree and not of kind. Thus we may make the graphs for rocks and water serve in a rough way for rock and air by imagining a few slight changes to be made. In the graph for the incident condensational wave, imagine the distortional-energy curve, $B_1$, to run up into practical contact with $A$ when the angle of incidence is about 60°, to remain very near to it till about 75°, and then to fall rapidly away to zero at 90°. At the same time, because of the great minuteness of the refracted energy, $A'$, the reflected condensational energy $A_1$ begins, at zero incidence, with practically unit value, and is to a very close approximation the inversion of $B_1$. In like manner, the incident distortional wave is, for incidences between 0° and the critical angle 35° 13′, practically represented by the two reflected waves. The reflected distortional energy begins and ends with value unity, passing through a small minimum value immediately before the critical angle is reached; while the reflected condensational energy begins and ends with zero and passes through a corresponding maximum which is practically unity. The refracted condensational energy, $A'$, is very small throughout, and could not be shown graphically with the others unless it were drawn to a scale of at least 1000 to 1. Immediately after the critical angle is past the condensational energy in

the air rises abruptly to the greatest value it ever attains, about 1/5000th of the whole, and falls off steadily with increasing incidences until it vanishes at grazing incidence. Practically the whole energy is retained in the solid in the purely distortional form.

The following example of the behaviour of waves at the plane surface of rock and fluid, whose density and compressibility are equal to those of the rock, may claim some attention as a case which may arise when lava or molten rock is in contact with solid material.

DISTORTIONAL WAVE INCIDENT IN THE ROCK

| Incident | | Reflected | | | Refracted | |
|---|---|---|---|---|---|---|
| $\phi$ | B | $B_1$ | $\theta$ | $A_1$ | $\theta'$ | $A'$ |
| 0 | 1 | 1 | | 0 | | 0 |
| 14° 2′ | 1 | ·725 | 25° | ·144 | 18° | ·131 |
| 26° 34′ | 1 | ·318 | 51° | ·261 | 35° | ·421 |
| 35° 13′ | 1 | 1 | 90° | 0 | 48° | 0 |
| 45° | 1 | ·147 | imaginary | | 66° | ·853 |
| 50° 44′ | 1 | 1 | ,, | | 90° | 0 |
| Higher incidences | 1 | 1 | ,, | | imaginary | |

CONDENSATIONAL-RAREFACTIONAL WAVE INCIDENT IN THE ROCK

| Incident | | Reflected | Refracted | | Reflected | |
|---|---|---|---|---|---|---|
| $\theta$ | A | $A_1$ | $\theta'$ | $A'$ | $\phi$ | $B_1$ |
| 0 | 1 | ·021 | 0° | ·979 | | 0 |
| 14° 2′ | 1 | ·007 | 10° | ·937 | 8° | ·055 |
| 26° 34′ | 1 | ·003 | 19° | ·839 | 15° | ·158 |
| 45° | 1 | ·104 | 32° | ·646 | 24° | ·256 |
| 59° 2′ | 1 | ·240 | 40° | ·503 | 30° | ·256 |
| 73° 18′ | 1 | ·268 | 46° | ·471 | 34° | ·261 |
| 84° 17′ | 1 | ·039 | 48° | ·618 | 35° | ·344 |
| 90° | 1 | 1 | 48°·2 | 0 | 35°·3 | 0 |

The chief peculiarity in the first of these cases is, perhaps, the vanishing at *two* incidences of the refracted wave in the fluid. It vanishes at the critical angle (35° 13′) at which the reflected condensational wave disappears; and then it has its own critical angle (50° 44′).

Between these limits its energy rises to a pronounced

maximum. In these respects there is a broad similarity between this case and the cases of rock and water, and rock and air. The differences are only differences of degree, depending on the different relations among the densities.

Some of the results given may be more easily understood if presented in the following way.[1]

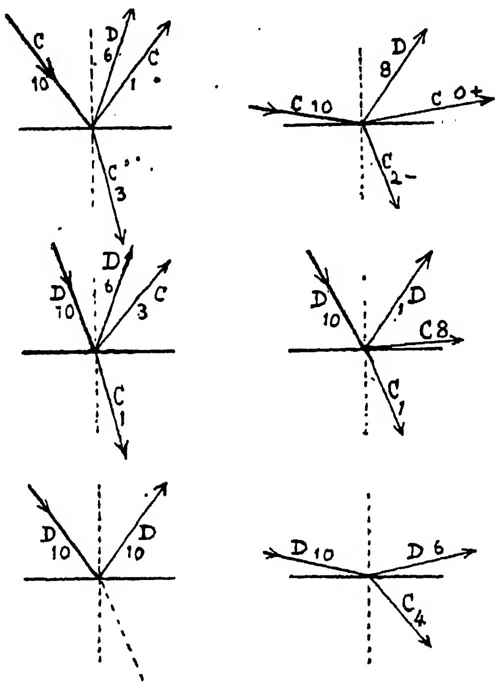



Fig. 32.

Each figure shows approximately the manner in which the energy of a particular type of wave at a particular angle of incidence is distributed among its derivatives of both types. The incident ray is represented by the broadest line passing downwards from left to right. The upper medium is rock, and the lower water. Since condensational waves only can exist in a fluid, there is never more than

[1] Taken from an abstract of an Address published in the *Proc. Roy. Soc. of Edin.*, vol. xxii, 1899.

one ray in the lower medium. In all the figures, $C$ represents a condensational-rarefactional wave, and $D$ a distortional. The first two figures show how an incident condensational wave breaks up into two reflected waves and one refracted wave. The angles of incidence are 36° and 80° respectively. In both cases the greater part of the energy is reflected in the distortional form, and in the second case the reflected condensational wave is practically non-existent. The numbers attached to the different rays indicate roughly the amounts of energy associated with them.

In the four remaining figures the behaviour of an incident distortional wave at various incidences is shown. The condensational wave travels faster than the distortional wave, and is therefore in all cases reflected at a greater angle. When the angle of incidence approaches 35° (see fourth diagram), the reflected condensational wave is sent off at a very high angle, and carries away with it most of the energy, about 80 per cent., while the amount of energy associated with the reflected distortional wave is excessively small. At a slightly greater angle of incidence, namely, 35° 16′, the reflected condensational wave passes off parallel to the surface with zero energy, and at higher incidences has no existence. With the vanishing of the reflected condensational wave at this critical angle, the refracted condensational wave also vanishes—a very remarkable result. Consequently at this angle all the energy is reflected back into the rock in the distortional form. See the fifth diagram, showing by a dotted line the *direction* which the refracted ray would have had if it had existed. For higher incidences the refracted ray comes strongly into evidence, accounting for approximately half the energy, but becoming less important at very high incidences, until at grazing incidence nothing is left but the reflected distortional wave.

In the paper in the *Philosophical Magazine* detailed calculations will be found for the more complex cases in which both the media are elastic solids through which the elastic waves are propagated with different speeds. The general nature of the results are, however, sufficiently indicated by the examples already given.

The main point is that when we have to do with transitions from one medium to another the character of the wave-motion may alter fundamentally. A comparatively simple type of vibration will speedily become transformed into a complicated combination of types. When we consider the heterogeneous nature of the crust of our earth we can have no difficulty in understanding the great complexity of earthquake movements. Even although the disturbance may for the greater part of its course have passed through fairly homogeneous material it cannot reach us except through the excessively heterogeneous structure familiar to the geologist. In this last stage of its progress it must become greatly altered from what it was originally.

If, again, different types of seismic motion can be distinguished at the surface of our globe, we seem compelled to assume that their separability depends upon their having traversed long stretches of fairly homogeneous material. It is easy to see that under such conditions the compressional waves in this approximately homogeneous portion would outstrip the distortional waves; and this separation in time of the first appearance of each type would be apparent at the surface, although the motions might not otherwise be distinguishable the one from the other. We shall return to this in the next chapter.

Another important point touched on in the above investigation is the possibility of barriers to the propagation of seismic disturbances. Any large dislocation or fault may stifle the movements, while the existence of total reflexion may greatly modify the transmission across a boundary when the manner of incidence is but slightly altered.

The earth's surface is itself a most effective barrier, a very small fraction indeed of the original energy being transmitted to the air. Thus, at whatever angle elastic disturbances may impinge internally upon the earth's surface, by far the greater part of the energy is reflected within the crust and is transmitted through it by a succession of internal reflexions until the energy of motion is frittered away as heat in virtue of the viscosity of the rock.

In the cases detailed above marked complications arise

in virtue of the (comparatively simple) theory of perfect elasticity. In real cases of transmission of earthquake disturbances we have to deal with motions which are only quasi-elastic. For the treatment of such kinds of motion our mathematical methods are not powerful enough. But we may be sure that these quasi-elastic disturbances will be accompanied by even more complicated changes than those indicated above.

The complexities of wave-motion are indeed quite beyond our powers of analysis. Once the displacements become greater than the limits within which a simple elastic theory is applicable we really do not know what may happen. To a first approximation the speeds of propagations are assumed to depend only on the elastic constants and the densities. Waves then become simply superposable. But in many, indeed in most, cases this simple superposability does not hold. Interference phenomena of a complicated nature are frequently present. Again the viscosity of the material must seriously affect the transmission of disturbances through it; and this viscosity will be more effective in the broken stratified and faulted crust than in the layers deeper down. The quicker vibrations will be killed out more rapidly than the slower vibrations. Different types of vibrations will coalesce to form wave groups whose apparent speed of propagation may not be the real speed of propagation of any single type of wave. Some of these more difficult questions have been considered by Nagaoka, who has attempted in this way to explain certain peculiarities of earthquake motion. Whatever immediate success may attend these attempts they are to be welcomed as tending towards the elucidation of the very difficult physical problems the seismologist must face sooner or later.

# CHAPTER XI

## UNFELT SHAKINGS OF EARTH

Minute Movements. Daily Oscillations of the Plumb. von Rebeur Paschwitz. Milne's Theory. Pulsations. Motions due to Distant Earthquakes. Comparison of Seismograms. Typical Earthquake Record. Preliminary Tremors. Large Waves or Principal Portion. Omori's Analysis of Periods. Interpretation of Record. Resonance frequent.

THE invention of the seismograph marked an important epoch in the development of seismological science. Some idea of the real movement of the ground then became possible. Automatic time recorders introduced an accuracy quite unattainable in the earlier days. In addition, however, to thus making more definite our knowledge of recognizable earthquakes, the instruments demonstrated the existence of motions and shakings of earth which were too small in their amplitudes or too slow in their periodicities to be felt by man.

The first seismoscopes to record unfelt movements were lakes and ponds. Thus the great earthquake of Lisbon, in 1877, was succeeded by rhythmic movements in Loch Lomond, Loch Ness, and in other lakes. These were of the nature of forced seiches attributable to unfelt oscillations of the ground transmitted from the earthquake source. The full significance of this observation was not realized until comparatively recent times.

In 1877, the Russian astronomer, Magnus Nyren, observed oscillations in the level of the axis of the transit instrument at Pulkova, and these he suggested as having been caused by an earthquake which had occurred 1 hr. 14 min. earlier at Iquique. Variations of level observed by other astronomers are now known to be referable to the same cause.

Meanwhile the Italian seismologists had constructed delicately swung pendulums, the practically ceaseless motions of which were viewed through a microscope. By

means of these Tromometers the earliest systematic investigations into the tremors and pulsations of our earth were entered upon.

Independently and along a different line of development, John Milne in Japan had constructed his horizontal pendulum (described in chapter iv), by means of which he began the study of various kinds of earth movements. He seems to have been the first to recognize what he called slow earthquakes, which could not be felt because of their slow periodic motion. As early as 1883 he ventured on the statement that 'it is not unlikely that every large earthquake might be with proper appliances recorded at any point on the land surface of the globe'.

This prediction began to be fulfilled six years later, when a record obtained at Potsdam was recognized as due to the same disturbance which produced a fairly strong earthquake in Japan.

Dr. E. von Rebeur Paschwitz had set up at Potsdam a delicate form of horizontal pendulum, with which he hoped to detect the gravitational influence of the moon. George Darwin, in England, had attempted to solve the same problem by means of a very delicate bifilar suspension. Both investigators found that there were so many movements and disturbances taking place that the lunar action could not be separated out. From a careful examination of the various types of records obtained, von Rebeur Paschwitz was led to identify certain of these with real earthquakes occurring at a distance.

When Professor Milne finally left Japan in 1893 he set up at Shide in the Isle of Wight the first seismological observatory ever established in this country. Hitherto, outside recognized seismic regions, the installation of instruments as seismometers had never been imagined. Now, through the agency of the Seismological Committee of the British Association, inspired by its secretary, John Milne, more than fifty horizontal pendulums have been set up all over the earth's surface; and from these every half-year there pour in to the central station at Shide numerous records of unfelt earthquakes.

We shall first consider briefly some kinds of motion recorded by delicate instruments of the horizontal pendulum type.

One of the most marked features is what is called the daily period. Daily oscillations of levels had been from time to time observed by astronomers; but the first to make a thorough systematic study of the phenomenon was von Rebeur Paschwitz, whose aim, as already mentioned, was to measure directly the gravitation action of the moon. About the same time Milne in Japan noticed the same phenomenon, by means of a light horizontal pendulum which had been set up to study tremors.

To study the phenomenon completely at least two horizontal pendulums must be set up side by side giving movements in perpendicular directions, say, north-and-south and east-and-west. In some observatories three are used, inclined at angles of 60° to each other. It is then found that the daily oscillations of the pendulums indicate a slow tilting movement of the ground, which might be described generally as a rising of the ground on the east and north sides from about sunrise till well on in the afternoon, followed by a reverse tilting during the hours of night. The range of tilt is generally greater in the EW. direction than in the NS. direction, and is more marked in the summer months than in the winter months. These are the broad features for stations in the northern hemisphere.

E. Mazelle, in an elaborate paper published in 1900,[1] has given a full account of the daily period month by month throughout a whole year. No doubt differences in detail would be given if another year of months were chosen; but the general character of the phenomenon will probably not change materially. I have therefore reproduced on a diminished scale the graphical representation of the results obtained by him with three Rebeur Paschwitz's horizontal pendulums at Triest. The instruments were installed in a cellar with a very small temperature variation, so that disturbances due to air currents were reduced to a minimum.

[1] *Mitt. d. Erdbeben-Kommission d. kais. Akad. d. Wiss. in Wien.*

From the photographic record given by each pendulum the deflections at the successive complete hour marks were read off and tabulated hour by hour and day by day throughout each month. Hourly means were then formed, and from these, corrected for non-periodic changes, numbers were obtained giving the average monthly daily periodical change for each month. The deviations from the mean of each set were finally reduced to angles of tilt, the reducing

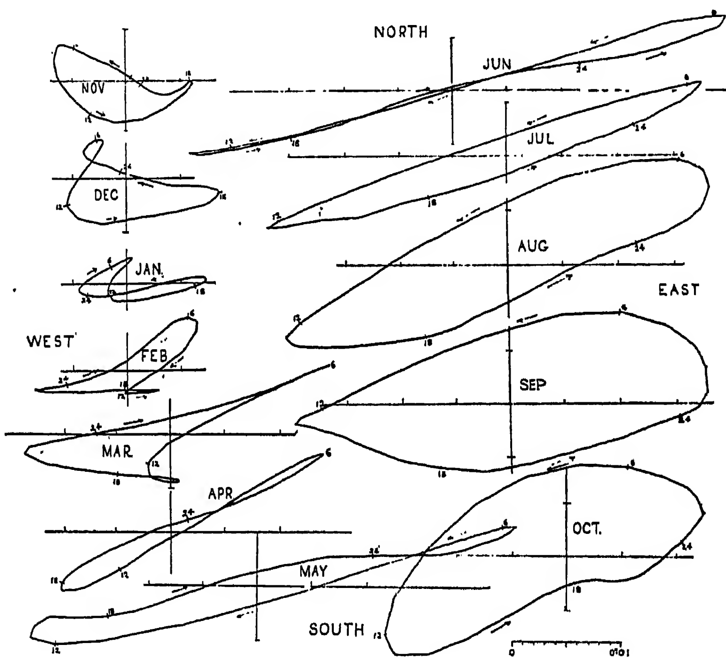



FIG. 33.

factor being obtained by measuring the period of swing of the pendulum. The resultant of the three tilts for every hour, after being determined in magnitude and direction, was laid off vectorially from a common origin, and a continuous curve drawn through their extremities. The exact significance of this curve may perhaps be best described thus. Imagine a thin rod to be fixed perpendicular to the level surface of the ground. Then, in consequence of the

daily fluctuation at present under consideration the extremity of this rod will describe relatively to the direction of the force of gravity a curve similar to the curves shown in Fig. 33, or, to speak more accurately, will describe curves day by day, the average form of which for each month will be as represented.

In each of the graphs midnight is represented by 24 and noon by 12, 6 and 18 representing the intermediate times. The arrow heads indicate the direction in which the graph is to be described.

It is evident at once that the oscillation during the colder months, November, December, January, and February, are much smaller than those of the warmer months, May, June, July, August, and September, while the oscillations during the remaining months are intermediate to the extremes. The peculiar double-looped form of the January graph contrasts strongly with the much simpler forms which are characteristic of most of the other months, and indicates a pronounced half-daily period associated with the daily period. This is also brought out by Mazelle's harmonic analysis of the periodicities.

In order to guard against too much attention being paid to the details of the graphs here shown it should be mentioned that some of these details are within the errors of observation. The greatest range is for the month of June and amounts to only one-fiftieth of a second of arc; and the errors of observation will affect the second significant figure of the number measuring the deflection.

There seems to be little doubt that the daily period is connected with the heating effect of the sun; but to explain the half-daily period we seem to require to call in the aid of tidal action.

The direct heating effect can hardly be considered as really efficient, for, ever since Forbes tested the propagation of solar heat downwards through the crust of the earth, we have known that the daily variations of temperature are cut out at the depth of a few feet, and that the annual variation does not penetrate more than 30 or 40 feet.

Milne has for many years argued that an important factor in the production of the daily oscillation is to be sought for in the differential loading and unloading of neighbouring areas by solar influences. In the daytime evaporation and transpiration from vegetation relieves the load, and the ground consequently rises where the solar action is more pronounced. This will generate a daily wave in the earth's crust facing the sun. Unfortunately this explanation does not seem to apply to the southward deviation which in the warmer months occurs simultaneously with the westward deviation. Milne has carried out a remarkable series of experiments on the effects of loading and unloading in the vicinity of a delicate horizontal pendulum, and many of the results fit in well with his explanation. For example, horizontal pendulums placed on the sides of a valley in the Isle of Wight indicate an increased steepness on those sides during heavy rain, and a diminishing steepness during fine weather. The idea is that the precipitation loads the valley, causing the bed to sink. During steady fine weather the movements at night correspond to those in wet weather, evaporation during the day causing unloading and the condensation at night the reloading. In other words, the valley opens slightly under the influence of the sun's heat and closes again during night. Similar effects had been previously observed in Tokyo.

In addition to the slow daily oscillation of the ground as measured or at least indicated by the horizontal pendulum, there are other movements more or less irregular which are usually referred to as tremors or pulsations. Some of these are in many cases due to convection currents set up in the air of the room or case in which the instrument is placed. Others, again, seem to be due to real pulsations of the ground set up possibly by winds or barometric changes not necessarily exactly in the locality where the instrument is. Unlike the daily period these pulsations seem to be more pronounced in the winter months than in the summer months. Thus Mazelle, in his discussion of the pulsations at Triest, leaves out of account the five months from May to September.

To show the nature of these pulsations I reproduce a part of one of the records obtained by Omori in Japan.

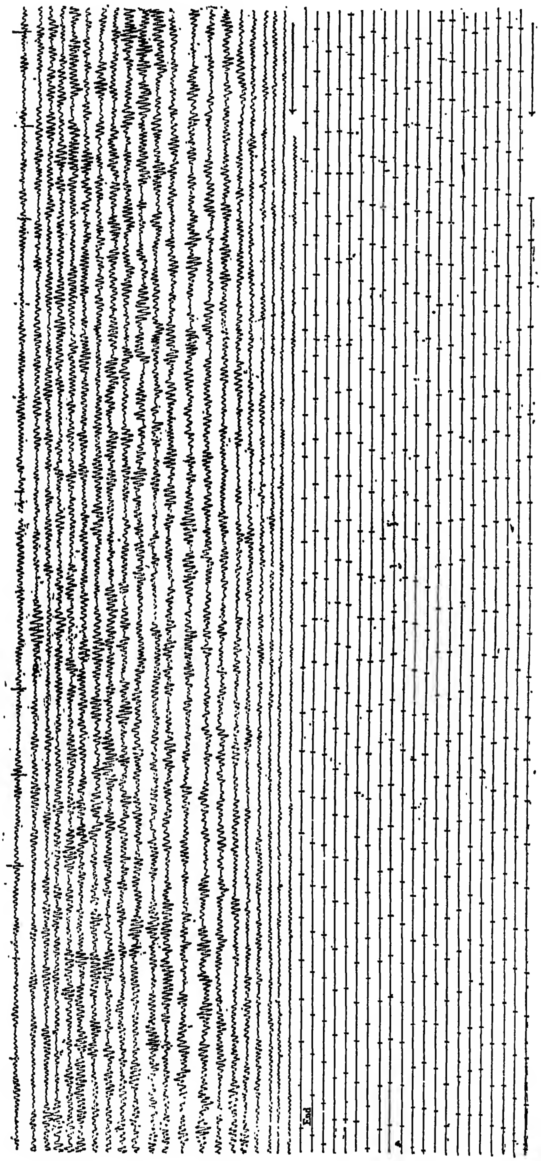



Fig. 34.—EW. Pulsations at Tokyo (Omori), Nov. 17–18, 1900.

The motions of which these records are magnifications

rarely exceed the tenth of a millimeter in range, and are usually much smaller. The periods can be estimated at once from the record, which contains a time scale marked minute by minute on the same sheet. In the part reproduced it will be seen that the crests and hollows run about nine to the minute. Omori finds that there are two distinct periods, the one ranging from 3·4 to 5·7 seconds, and the other from 7·1 to 9·3 ; and he points out that these periods are particularly prominent in the tremors which come from distant earthquakes. The suggestion is that they are the periods of vibration natural to the plain in which Tokyo lies.

Attempts to connect these pulsations directly with definite meteorological conditions or changes either at the locality itself or in the neighbourhood have not been altogether successful. Probably several causes are at work modifying each other's action, such for example as cyclonic depressions, barometric gradients, high winds blowing on mountain chains, changes of humidity and rainfall, and even real seismic disturbances too feeble to be felt. Wiechert has suggested the breaking of sea waves on the coast as a cause of pulsations.

We now pass to the consideration of the propagation of earthquake disturbances to great distances.

When the first records of moderate earthquakes were obtained in Japan by Milne, Gray, Ewing, and others, the large motion was seen to be heralded by smaller but more rapid vibratory motions, which, from their appearance on the seismogram, were called 'ripples'. These ripples I explained in my paper of 1888 as being in all probability due to the elastic vibrations started by the earthquake and propagated through the earth's crust at a quicker rate than the quasi-elastic vibrations which form the most conspicuous part of the felt earthquake. This explanation necessarily implies that the further away an observing station is from the seismic source the longer will the ripple portion of the seismogram be. The beautiful seismograms obtained within recent years by Omori by means of his horizontal pendulums installed in Tokyo bring out this feature in a very satisfactory manner.

To exhibit it and at the same time lead up to the characteristics of the seismograms of unfelt earthquakes, I reproduce, on a diminished scale, parts of five seismograms of earthquakes which, originating at different distances from Tokyo, were felt there as weak shocks. The date and time of occurrence of each, and the distance and direction from Tokyo of the probable position of the epicentre, are given in the following table :

| Date | Time at Tokyo | | | | Epicentre's Distance from Tokyo | Epicentre's Direction from Tokyo |
|---|---|---|---|---|---|---|
| 23/4/01 | 3 h. | 8 m. | 30 s. | a.m. | 13 km. | S. 15° E. |
| 8/5/04 | 4 | 23 | 49 | a.m. | 154 | N. 33° W. |
| 31/1/01 | 10 | 42 | 58 | a.m. | 680 | N. 33° E. |
| 24/6/01 | 4 | 6 | 19 | p.m. | 1020 | S. 44° W. |
| 25/8/04 | 6 | 2 | 31 | a.m. | 1030 | ,, ,, |

The initial portions of the corresponding seismograms are reproduced on one sheet, the scale of minute intervals being shown alongside ; and the gradual lengthening of the preliminary ripples or tremors as the distance of the origin from Tokyo increases is evident at a glance. In the seismogram of April 23, 1901, there is a well-marked undulation superposed upon the more rapid oscillations. This feature may be seen, although not so clearly marked, in the other diagrams. These undulations are no doubt due to the natural swing of the horizontal pendulum started by appropriate impulses. As a rule, they die out before the more rapid movements disappear, showing that there is no continued synchronous motion in the ground itself. They decay because of frictional resistances in the instrument. As compared with the more rapid movements the slow undulations are more marked the nearer the origin is to the place of observation. This is quite in accordance with what we should expect ; for the nearer the origin the greater the chance of an appropriate impulse acting on the instrument.

These and other similar cases have been carefully analysed by Omori, who indicates where he considers the second kind of preliminary tremors to begin. It is not always easy to see what has guided him in marking the position of this

point; and it is doubtful if any such distinction would have been imagined if we had not had the records of the unfelt

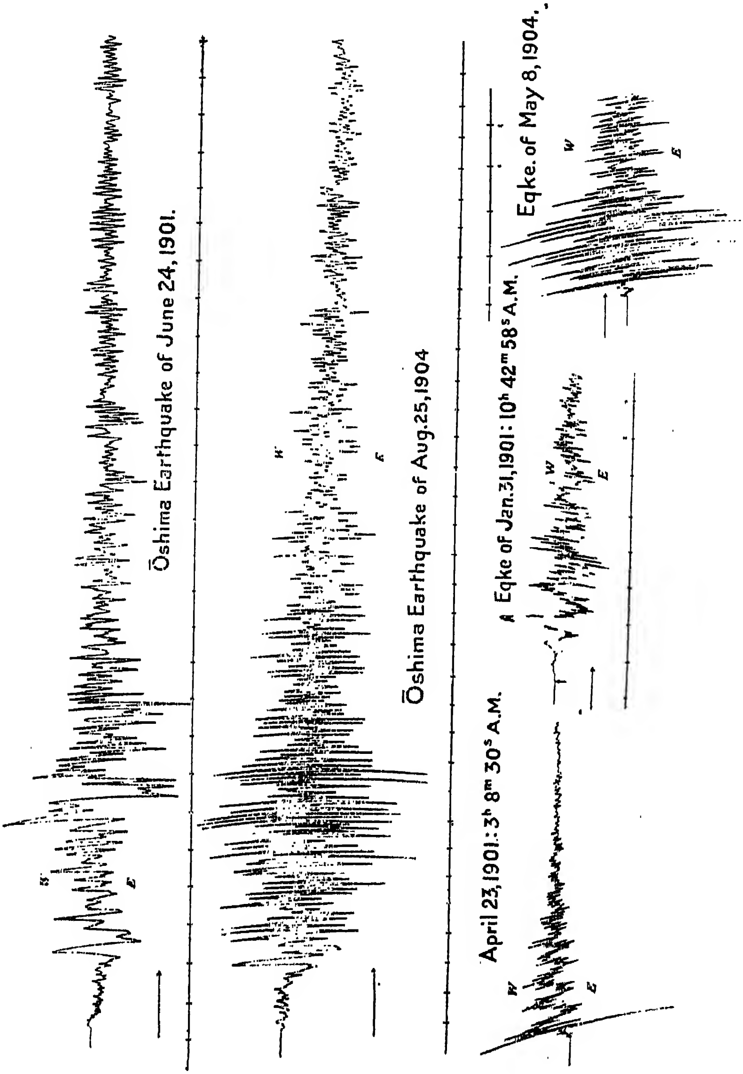



FIG. 35. RECORDS OF EARTHQUAKES FELT IN TOKYO.

motions of distant earthquakes to guide us. To the study of these records we now pass.

First let us look at two other records[1] obtained by Omori in Tokyo. These are due to earthquakes which originated, the one in Ceram, and the other near New Guinea. Nothing was felt in Tokyo at the times these records were being traced out on the revolving cylinder. The instrument which recorded them was the same as that used to record the movements of the weak felt earthquake of Jan. 31, 1901 (Fig. 35). The multiplication was 10, giving for the greatest range of movement of the heavy weight on the pendulum boom the following values: 11·1 mm. in the shock of Jan. 31; 8·7 mm. in the Ceram shock of 1899; 4·3 mm. in the New Guinea earthquake of 1900. It may be mentioned that other local shocks which were felt gave on the same instrument ranges of only 5·6, 5·2, 3·3, and 2 mm. The reason why these were felt, whereas the Ceram and New Guinea shocks were not felt, was largely because of the shorter period or quicker motion of the vibrations propagated from the nearer origin.

According to Omori, the epicentres of these two earthquakes lay respectively 4410 kms. S. 15° W., and 5570 kms. S. 15° E. of Tokyo. As will be seen at a glance, the preliminary tremors which usher in the large movement are extended over a much longer interval of time than in the case of the five felt earthquakes already discussed. The following are the estimates for the times of duration of the preliminary tremors or ripples in these various cases. The first row gives the date of the earthquake in Omori's list of 1905; the second the distance of each epicentre from Tokyo in kilometres measured along the surface of the earth; the third the time of duration in minutes of the preliminary tremors; and the fourth the ratio of the distance to this time of duration.

| Date of Earthquake | 23/4/01 | 8/5/04 | 31/1/01 | 24/6/01 | 30/9/99 | 29/7/00 |
|---|---|---|---|---|---|---|
| Distance in kms. . | 13 | 154 | 680 | 1020 | 4410 | 5570 |
| Duration in min. . | 0·2 | 0·3 | 1·5 | 2·3 | 9·4 | 12·3 |
| Dist./Dur. . . . | 65 | 513 | 453 | 443 | 470 | 453 |

With the exception of the first case all these give very

[1] *Publications of the Earthquake Investigation Committee*, No. 21, 1906.

similar results for the rate at which the preliminary tremors outrace the larger movements.

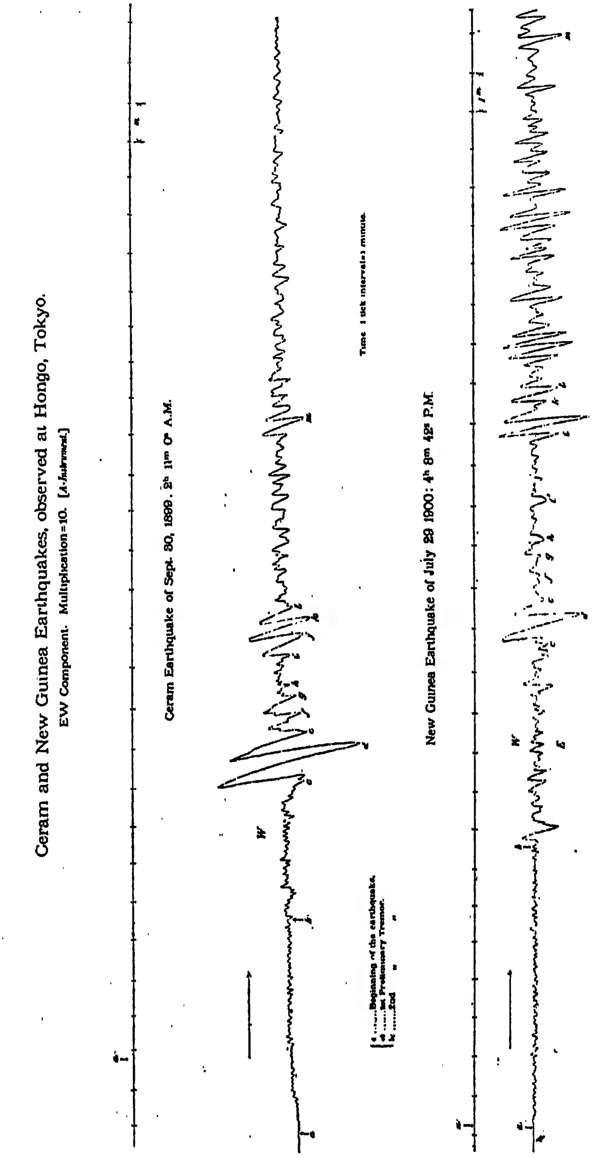



Fig. 36. Omori's Horizontal Pendulum Records, reduced 1/3.

Having thus, by consideration of the most recent results,

bridged over the interval between the felt earthquake and the unfelt, or, as Milne originally called it, the slow earthquake, let us look back at the growth of the new ideas which have within ten years transformed the study of seismology. Since the publication in 1899 of Milne's book on Seismology, in which these ideas were first systematically discussed, data have accumulated at an ever increasing rate.

The seismologist is never left long without a reminder that the 'solid ground' on which man builds is in frequent commotion. A new improved seismometer is hardly installed before a record is obtained of a fleeting oscillatory movement, the far-off couriers of a more or less violent quake. Milne's indefatigable energies, working through the Seismological Committee of the British Association, have resulted in the establishment of fully fifty seismological stations all over the earth's surface. At each of these the Milne Horizontal Pendulum is set up; and every half-year there flows in upon the central station at Shide in the Isle of Wight a converging flood of records. If we add to these the published records of the important seismological observatories in Italy, Austria, Germany, Russia, and Japan, we have a steadily increasing mass of material ever ready for discussion.

It is now established beyond the possibility of doubt that from an earthquake centre disturbances of various types spread out and travel with different speeds through the body of the earth and over its surface. Views differ somewhat as to the manner and lines of propagation; but as regards the preliminary tremors which form the first part of every record of an unfelt earthquake we must regard them 'as due to the propagation along the path of shortest time of certain elastic vibrations set up by the initial movements at the earthquake focus'.[1] The phrase 'unfelt earthquake' is a convenient expression, indicating that we are dealing with a mechanically recorded surface movement too small in amplitude and too long in period to be appre-

[1] See 'The New Seismology', *Scottish Geographical Magazine*, 1899.

ciable to our senses at the place where the record is taken, but too small only because we are not near enough to the origin where the disturbance was in all likelihood a violent earthquake.

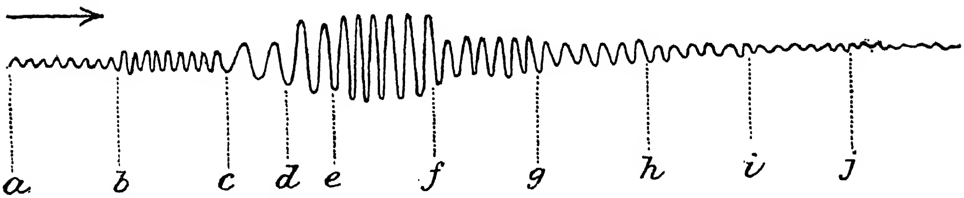
The early division of the record was into Preliminary Tremors and Large Waves; and this division still holds, but further study has led to the recognition of subdivisions of these. Although Rebeur Paschwitz had suggested the possibility, Oldham,[1] of the Geological Survey of India, was the first clearly to establish the existence in the complete record of two distinct phases in the Preliminary Tremors. These will be distinguished as *P* and *S*. The entry of the Second Phase is usually marked by a sudden increase in the amplitude of the sinuous record, and in many cases by a longer period. There is frequently some difficulty in deciding as to the exact instant at which either phase makes its appearance. Similarly there is in many cases a lack of precision in the time of commencement of the third phase, which marks the initial stages of the Large Waves, or Principal Portion as Omori prefers to call it. These points should be kept in mind when deductions are being made as to the speeds of propagation of the various kinds of motion.

But although there may be difficulty in always determining the initial stages of the Large Wave portion, there is no doubt as to its marked distinctiveness as a whole. It consists usually of a series of groups of undulations, each group attaining a maximum and being separated from the contiguous groups by intervening minima. After the last pronounced maximum the Principal Portion tails off into the End Portion, marking the gradual decay of the earthquake disturbance.

From a careful study of the seismograms of unfelt earthquakes obtained in Tokyo, Omori divides the Principal Portion into a number of phases, each of which is characterized by the presence of a fairly constant period and a fairly uniform amplitude. I reproduce a sketch of his

[1] 'On the Propagation of Earthquake Motion to Great Distances,' *Philosophical Transactions*, vol. cxciv, A. 1900.

typical earthquake diagram and add his own description of some of the phases.




*a b*......1st Prel. Tremor
*b c*......2nd " "
*c d e f..j* ..Principal Portion ....
*j*......End Portion

*c d*......1st Phase of princ. portion.
*d e*......2nd " " "
*e f*......3rd " " "
*f g*......4th " " "
&c.

FIG. 37.

'The Principal Portion denotes the most active part of an earthquake, which follows the preliminary tremors, and consists of movements of larger amplitude. The earlier part of the principal portion is further subdivided into three successive stages as follows: (*a*) the first phase, consisting of a few very slow undulations; (*b*) the second phase, consisting of slow undulations whose period is somewhat shorter than in the first phase; (*c*) the third phase, consisting of vibrations of period much quicker than in the preceding two phases. The third phase is followed by others of smaller amplitude which may be termed the fourth, fifth, sixth . . . phases of the principal portion.'[1]

As examples of real records we may take the initial parts of the Ceram and New Guinea earthquakes as recorded in Tokyo and reproduced in Fig. 36; also Omori's records of the great Indian earthquake of 1905, and the San Franciscan shock of 1906, shown in Fig. 38. On the same sheet I have reproduced Davison's record of the Indian earthquake, obtained at Birmingham on a horizontal pendulum of Omori's type. These are reduced three-sixteenths from the originals. A careful study of the diagrams will impress upon the mind the characteristics of the unfelt earthquake record much more thoroughly than pages of description.

[1] *Publications of the Earthquake Investigation Committee*, No. 13, 1903.

The important points brought out by these seismograms

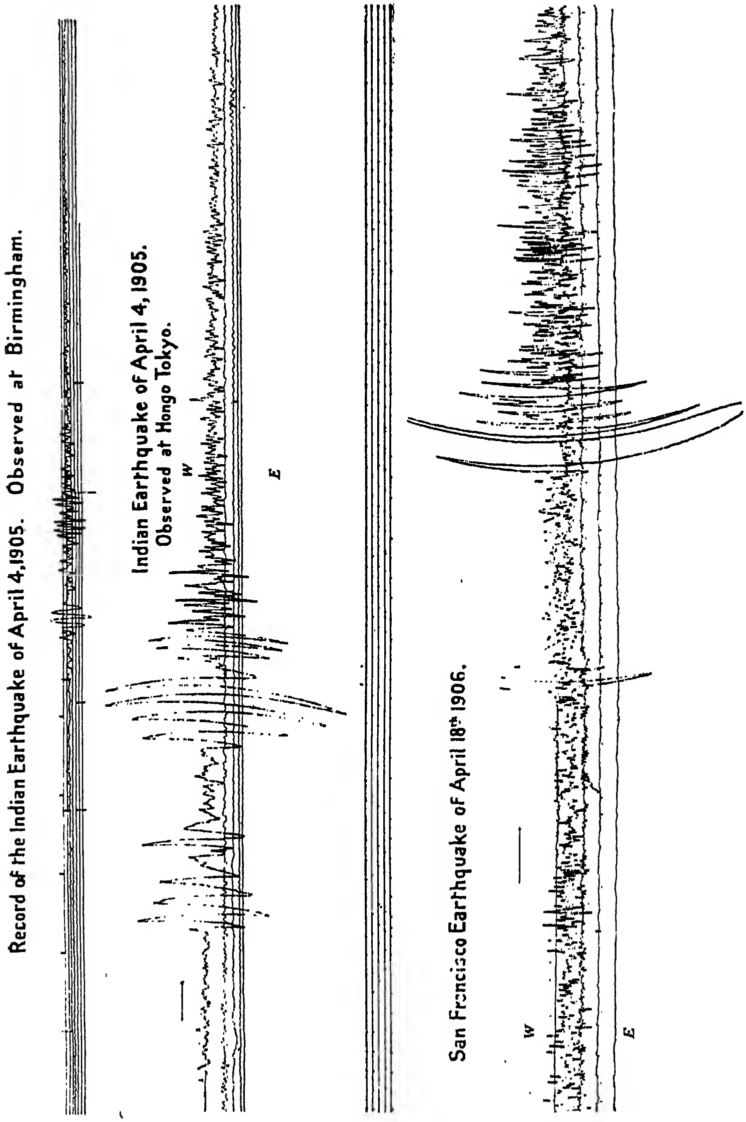



Fig. 38.

will be referred to as they arise. The time scale is given

by small vertical marks, one minute intervals in the Tokyo seismograms, five minute intervals in the Birmingham diagram.

The records obtained on Milne's horizontal pendulum are on a much less extended time scale. The instrument, however, admirably fulfils the purpose for which it was contrived, namely, the detection of the unfelt earthquake and the times of the important phases. Where, as in Milne's own observatory at Shide, comparisons are possible between the records of different types of horizontal pendulum, the more massive type does not seem to have any marked advantage over the other in indicating the beginning of the record, or in indicating the advent of the large waves. In details, however, there are considerable differences, not only between records obtained by instruments of different type, but also between records obtained by instruments of the same type. The elaborate instruments now being installed in first class observatories may in time lead to results commensurate with their cost; but so far their indications have mainly corroborated those given by the earlier and less elaborate forms.

As regards the character of the record, probably no one has analysed it more minutely than Omori. A first glance at any one of the records reproduced suggests an almost infinite variety of period and amplitude in the sinuosities traced out; but a little attention shows that there is a tendency to the recurrence of certain periods. According to Omori's careful and laborious analysis not only is this the case, but there is also a tendency for certain periods to be more characteristic of certain phases than of other phases. The following table taken from his paper[1] will indicate better than pages of letterpress the real meaning to be attached to the general statement quoted above in reference to the typical earthquake diagram. The results are average periods measured directly from the Tokyo records of eleven large earthquakes originating in Alaska, Asia Minor, Mexico, West Indies, Central Asia, and East Indies.

[1] *Publications of the Earthquake Investigation Committee*, No. 13, 1903.

PERIODS OF VIBRATION IN SECONDS IN THE VARIOUS PHASES OF THE EARTHQUAKE RECORDS OF ELEVEN UNFELT EARTHQUAKES

| Preliminary Tremors | | Principal Portion of Large Waves | | | | | | End Portion |
|---|---|---|---|---|---|---|---|---|
| Phase 1 | Phase 2 | Phase 1 | Phase 2 | Phase 3 | Phase 4 | Phase 5 | Phase 6 | |
| 1·5 (1) | | | | | | | | |
| 4·1 (6) | 4·8 (2) | 2·9 (1) | | | | | | |
| 7·8 (10) | 8·2 (9) | 8·7 (2) | 9·9 (3) | 10·1 (1) | 11·7 (3) | 9·5 (2) | 8·1 (1) | 9·9 (6) |
| 13·9 (5) | 15·0 (5) | | 14·1 (1) | | 14·9 (5) | 14·3 (6) | 14·5 (4) | 14·3 (3) |
| 18·0 (1) | 19·6 (1) | | | 20·4 (9) | 19·9 (2) | | | 19·8 (4) |
| | 24·8 (3) | | | 24·0 (3) | | 25·0 (1) | 25·0 (1) | |
| | 30·4 (2) | | 27·4 (5) | | | | | |
| | | 36·1 (3) | 33·7 (8) | 34·3 (1) | | | | |
| 40·3 (1) | | 44·6 (2) | 42·7 (3) | | | | | |
| | | 54·0 (2) | | | | | | |
| | | 66·0 (1) | | | | | | |

The figures in brackets after the number giving the period show the number of earthquakes in whose records that particular period was detected. If for the moment we regard six cases out of eleven as establishing a 'character', we may say that the preliminary tremors are characterized by a period of about eight seconds, the second phase of the large waves by a period of thirty-four seconds, the third phase by a period of twenty seconds, the remaining phases by a period of fourteen to fifteen seconds, and the end portion by a period of ten seconds. If we take the so-called fourth, fifth, and sixth phases as belonging to the End Portion, then the characteristic period is still about fifteen seconds.

Again, a broad glance at the general aspect of the table shows that the shortest periods are met with in the first preliminary tremors, that there is a tendency for somewhat longer periods in the second preliminary tremors, that the longest periods are encountered in the initial phase of the large waves, and that the end portion is characterized by periods intermediate between the shortest and longest.

Similar broad features are given by the analyses of other groups of earthquake records, with, however, considerable variations in detail. Thus, out of a group of eighty-four records of unfelt earthquakes (including five of the eleven

mentioned above), the periods which were met with in more than twenty of these were as follows :—

| | | |
|---|---|---|
| 4·6 sec. | 26 times | in First Preliminary Tremors. |
| 8·7 „ | 28 „ | in „ „ „ |
| 9·3 „ | 32 „ | in Third phase of Principal Portion. |
| 9·6 „ | 32 „ | in End Portion. |
| 13·6 „ | 22 „ | in Third phase of Principal Portion. |

From this analysis of one of Omori's tables we may conclude that there is a comparative constancy of period characterizing the first preliminary tremors, the third phase of the large waves, and the end portion ; but that there is not the same constancy of period in the other phases.

Omori has also discussed in detail the comparative magnitudes of the motions in the various phases. We should expect that on the average the larger amplitudes in the motion of the ground should be associated with the longer periods. This, broadly speaking, is what is established by Omori's analysis, that is, if we assume that the amplitudes given on the seismograms are really proportional to the amplitudes of the motions of the ground which are of course the original cause of the record. But as we saw in the chapter on seismometry this assumption is dynamically unsound when the seismometer is not powerfully damped, and is in the case of free swinging seismometers approximately valid only when the vibrations of the ground are distinctly more rapid than the free oscillations of the instrument.

Bearing in mind the conclusions come to in the discussion of Seismometry, let us consider some of the outstanding features of the seismograms obtained both on Milne's and Omori's forms of horizontal pendulums. From our present point these differ essentially in the period of swing. In Milne's instrument the period is about 12 or 15 seconds, whereas in Omori's instruments the period may be as long as one minute, and is never shorter than half a minute.

I am indebted to Mr. Heath of the Royal Observatory at Edinburgh for three records (Fig. 39) obtained with the Milne Horizontal Pendulum installed there. Two of these are reduced copies of the seismograms of the destructive

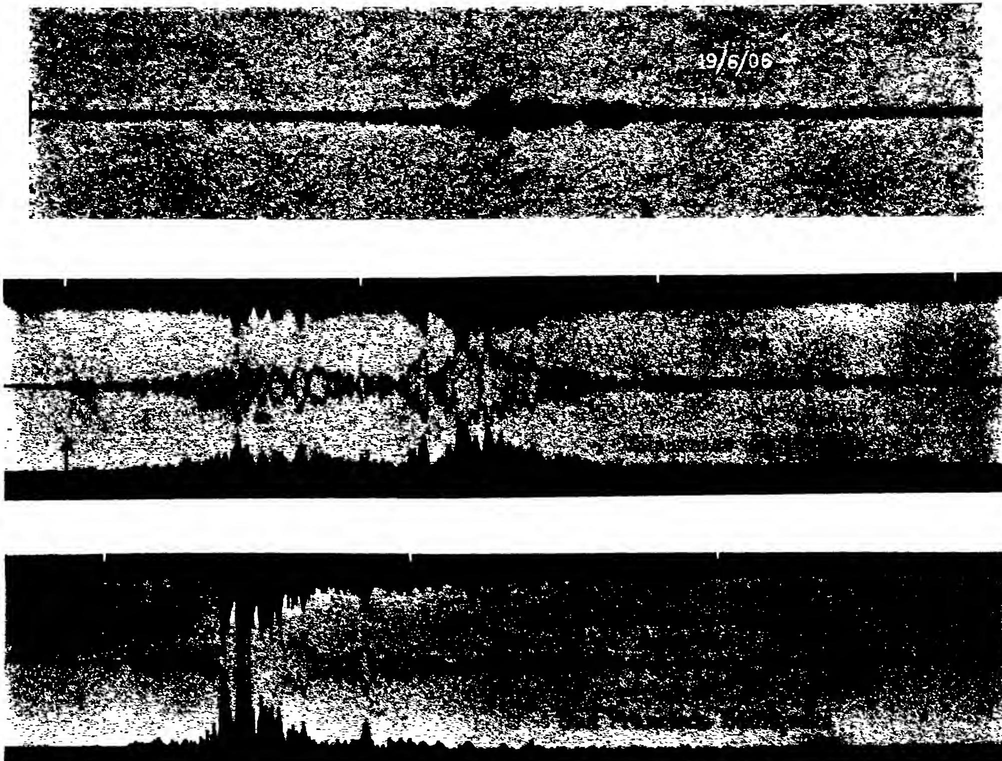



FIG. 39. EDINBURGH RECORDS OF SEVERAL EARTHQUAKES.

earthquakes of San Francisco and Valparaiso, the latter being mixed up with the record of an almost simultaneous North Pacific shock. The hours are marked by short white strokes. The third is an enlargement of a curious record of a small disturbance, which brings out very clearly the effect of resonance. The part reproduced is exactly an hour's interval.

About 10 minutes after the first faint tremor the record begins to assume a remarkably regular wave-like and approximately sinusoidal form, with a period of about half a minute. The gradual way in which the amplitude increases and then falls away is exactly what would be produced by the influence of a steady periodic force of approximately double the period of the pendulum. After this has gone on for five or seven minutes there is a change of period in the forced vibration. As this change is in the direction of an approach to isochronism the resonance effect becomes more powerful. This second vibration has a period of about 18 seconds. Finally, with a still shorter period of about 13 seconds the amplitude of the record opens out much more rapidly, and falls off again exactly as Galitzin's curves open out and contract (Fig. 21, p. 73). If any definite meaning is to be attached to the details of a seismic record, there seems to me to be little doubt as to the interpretation in this particular case. There has been a slow regular pulsation of the ground (with or without tilting—it matters not) followed by a somewhat more rapid vibration, and that again followed by one still more rapid. Each has had time to produce resonance effects on the instrument, so that we cannot regard the amplitudes as accurately corresponding to the amplitudes of the movements of the ground. As the period of the ground's motion approaches that of the free swing of the pendulum the resonance works up more quickly, the record swelling out more distinctly as the period becomes shorter. What is seen to be taking place in this special case will take place in the more complex cases when the motions are large and irregular. When for a few complete periods the ground is moving to and fro with a fairly approximate rhythmical motion, then, especially if the period is not very different

from the free swing period of the horizontal pendulum, resonance will be in evidence, and the horizontal pendulum will be itself set swinging, with an amplitude increasing at first and then diminishing afterwards. Thus the seismogram record will be a superposition of the real motion of the ground and the forced vibratory motion of the instrument. A glance at the seismograms reproduced in this book will disclose many cases of the kind. When the record, beginning small, swells out in a perfectly regular fashion and then dies away again, we may be certain that this represents not the real motion of the ground, but rather the forced motion of the seismometer. This is what has happened in the special case under discussion. The period of free swing of the horizontal pendulum installed at Edinburgh is 15·3 seconds, and any period from 10 to 20 seconds will undoubtedly produce, if continued long enough, a time variation in the amplitude of the kind indicated in the seismograms.

If alongside any particular horizontal pendulum we place another with a much longer period of free swing, the records, as Imamura showed by direct comparison, will differ distinctly in detail. The reason is obvious. Seismic periods which will produce evident resonance effects in the pendulum of shorter period will have nothing like the same resonance effect on the pendulum of longer swing. Compare for example the record at Edinburgh of the San Francisco shock (Fig. 39) with the record obtained at Tokyo on Omori's instrument with a period of 43 seconds (Fig. 38). A strict comparison is not possible, seeing that the stations are so far apart. But the character of the more open record with the more delicate instrument is altogether different from that of the less delicate seismometer. In fact, the great majority of the periods fall considerably short of 43 seconds, and hence the quicker vibrations of the ground will be more faithfully recorded on the less stable instrument.

Nevertheless this instrument also shows in its records evidences both of free swing and of resonance. And the same holds for the record of the Indian earthquake of 1905, which was taken when the instrument had a period of swing

of 62 seconds. The records are shown much reduced on the same sheet (Fig. 38). There can be little doubt, I think, that the very large amplitudes shown in both these records are mainly instrumental, for the simple reason that their periods are almost exactly the periods of free swing of the pendulum in each case. Take for example the Indian earthquake. In the first preliminary tremors we see not only rapid vibrations of a few seconds, but also coexisting oscillations whose periods are not far removed from one minute. In fact the pendulum is trying to swing its own natural swing, and is prevented from doing so in a steady manner simply because of the complex succession of impulses acting on it. Still the free swing, although somewhat broken, is there. Then at the point where Omori considers the second preliminary tremors to begin, a set of three large waves is produced and the periods of these are not very different from one minute. Evidently the time integral of the impulse acting just at the beginning of this interval has fitted in with the natural swing of the pendulum so as to produce a sudden and large resonance, and thereafter the free swing so started simply decays in time, because of frictional resistances and because of the action of forced vibrations not quite in tune. In like manner, some 9 minutes later, the motion of the seismograph pointer begins to work up again exactly as shown in Galitzin's curves on page 73. Now if we turn to the record of the San Francisco earthquake, we find very similar results, except that, the period being somewhat shorter, the large wave-like records appearing from time to time have periods approximating to that very period. The very large motion marking the presence of the Principal Portion might easily enough be explained as due to a slow tilting movement, producing a large displacement on the boom. The subsequent movements are partly due to the decaying motion after the great swing, and on them we see appearing abrupt jagged wavelets which persist for some ten minutes and are followed by a more uniform motion having all the characteristics of a forced vibration with resonance effects. All through the record the same phenomenon can be traced, not so evidently as in the records of the more quickly

swinging pendulums of Milne's type of instrument, but none the less certainly. The conclusion seems to be that we can place little reliance on the indications of amplitude. An appropriate impulse may set up large natural swinging in the pendulum, and when that is done it is impossible to say anything as to the real amplitudes of the vibrating ground. A comparatively small movement of the ground, with an appropriate period, may produce on the instrument a greater effect than a much larger motion of the ground when, with that motion, a very different period is associated. A consideration of the highest full line curves in the figure on page 80 shows that when the period of the forced vibration increases from one-third to one-half of the free swing period of the pendulum the effect of resonance will augment the amplitude in the ratio of 13 to 11; and still greater augmentations will occur as the period gets nearer the free swing period of the pendulum. Even when the forced vibration period is not by any means particularly near in value to the period of natural swing of the pendulum, a distinct resonance effect is nevertheless produced. It may be laid down as a general principle that, for periods of forced vibration varying from one-third to one-and-a-half times the period of free swing of the horizontal pendulum, the effects of resonance on the amplitudes are such as to preclude us entirely from drawing any sure conclusions as to the real magnitude of the earth movements.

As regards the periods, however, it is quite another matter. These are reproduced more or less faithfully in the seismogram.

Two interesting remarks are made by Omori in regard to the periods which seem to characterize the different phases of the motion (see above, p. 203). He finds that the period of 4·6 seconds, which is conspicuous in the preliminary tremors of records taken in Tokyo, is also a characteristic period very prevalent in the pulsations of the ground referred to on page 193. The suggestion is that this period belongs as much to the stretch of rock and soil on which Tokyo stands as to the original disturbance which has come from a distance. The disturbance, in fact, throws the region into

its own natural state of vibration as well as forcing on it other periodicities. This view is dynamically sound, and is quite in line with what has been already said regarding the influence of forced vibrations on the instrument. The original earthquake motions produce forced and free vibrations in the parts of the earth's crust which they invade, just as the primary and secondary movements of the ground produced in any locality act upon the seismometers installed there.

Another feature pointed out by Omori is the fact that seismograms obtained in Tokyo closely resemble one another when the original disturbance has come from the same region. If this should be corroborated for all centres of seismic disturbance, then the seismologist will be able to recognize from the appearance of the record the source of the shock which has produced it.

# CHAPTER XII

## SEISMIC RADIATIONS

Identification of Phase. Relation between Period and Amplitude. Selected Data of Kangra Earthquake (1905). Decay of Intensity with Distance. Early Tremors killed out. Milne's Method of Fixing Origins. Omori's and Laska's Modifications. Milne's World-shaking Earthquakes. Milne's and Oldham's Time Graphs. Imamura's Statistical Discussion. The Guatemala Earthquake (1902). Oldham's Speculations. General Comparison of Times of Transit. Arcual Transmission of Large Waves. Approximate Chordal Transmission of Tremors. Fisher's Hypothesis of Two Waves in Liquid Interior. Evidence as to Longitudinal and Transverse Vibrations. Nagaoka's Stratum of Maximum Velocity. Angles of Emergence measured by Schlüter. Brachistochronic Paths: Kövesligethy, Rudzki, Knott, Benndorf. Mathematical Theory compared with Observation. Poisson's Ratio satisfied. Energy Distribution. Group Velocities. Anomalous Dispersion (Nagaoka). Lamb's Vibration of Elastic Sphere. Jeans, Rayleigh, and Love on Stability of Earth.

JUST as a source of light is a disturbance which spreads out as luminous radiations through the ether, so an earthquake is the origin of seismic radiations through the body of the earth. In studying the manner of this radiation we must compare the records at different stations of one and the same earthquake.

The general character of the earthquake record has been considered in the last chapter. Except in very exceptional cases the record is characterized by a series of smaller quicker movements known as the Preliminary Tremors, followed by larger slower movements known as the Large Waves. Omori has given a more detailed classification into various phases, basing his division partly on amplitude of movement, partly on the periods (see above, p. 200). For present purposes, however, it will suffice to recognize the First and Second Preliminary Tremors and the Large Waves as the great characteristic phases of the record of unfelt earthquakes.

When the record is obtained at a station close to the origin it is not always possible to distinguish the two phases of the preliminary tremor; and the large motion follows on in less than a minute. At an earth's quadrant distance from the origin the preliminary tremors last some thirty minutes before they are overwhelmed in the greater motion of the large waves. The conclusion is obvious. We are dealing with a complex set of disturbances which originate at the seismic focus and radiate outwards in all directions with very different speeds of propagation.

To determine these various speeds of propagation we must be able to identify the corresponding phases of the seismogram obtained at different stations. But this identification is not such a simple matter as might at first be supposed. Already we have touched on some of the difficulties of interpretation of the records given by free swinging seismometers. Especially disconcerting is the effect of resonance, which is not confined as generally imagined to cases in which the forcing vibration has a period nearly equal to that of the free swinging instrument, but is present through a wide range of periods. Looking back to the formula given in chapter v, p. 77, we see that, when a slow swinging horizontal pendulum is recording a rapid vibration of the ground, the amplitude of the forced vibration depends not only on the amplitude of the forcing vibration, but also on its period.

Thus if $f$ is the amplitude of the forcing vibration of period $2\pi/p$, and $a$ the forced vibration amplitude imposed upon the instrument by a sufficient succession of periodic vibrations, then when $2\pi/p$ is distinctly less than $2\pi/n$, which depends on the free vibration period of the instrument, the relation between $a$ and $f$ is given approximately by the formula

$$a = f/p^2 = fT^2/4\pi^2$$

where $T$ is the forced vibration period.

That is to say, the mere increase in the period of the forcing vibration is sufficient to increase the registered

amplitude on the instrument, although the motion of the ground may not have increased.

No doubt the irregularity of the earthquake motion will, except in very exceptional cases, prevent this effect from developing to the full extent indicated by theory. Nevertheless, when the dominant period characteristic of a particular phase becomes longer, the tendency must be for a greater amplitude to declare itself. Omori, from a careful study of seismograms, has established that the amplitude does on the whole increase with the period throughout a given earthquake record. What theory teaches us in this respect is that the amplitude of the record may be largely determined by the period of the movement, especially in the preliminary tremors.

In his interesting analysis of the relation of period and amplitude in the records of the Kangra earthquake of 1905, Omori concludes that the amplitudes associated with the different periods increase with the period, reaching their average maximum when the period is about 45 seconds. This is in accordance with the theory of resonance developed in chapter v, if we assume that 45 seconds is not far from the average of the free vibration periods of the various instruments whose records supply the material. In forming his averages, Omori leaves out of account the very large deflections which are evidently due to proper pendulum oscillation. The effects of resonance, however, show themselves through a great range of periods and not merely in the immediate vicinity of the natural free vibration period of the seismometer.

There is as a rule no difficulty recognizing the advent of the large waves, and especially that phase of it which Omori distinguishes as the third. It seems to mark the climax of the movement and to correspond to the period during which the energy is most developed. At the same time, particularly when the records obtained by different instruments are to be compared, the effects of resonance will complicate the problem. The maximum amplitudes, depending partly on the real amplitudes of earth movements, partly on their periods, and partly on the periods

of the free swinging instruments, will occur at different parts of the corresponding phases. Occasionally seismograms of the same earthquake as given by different instruments bear a striking resemblance to one another; but this is not usually the case. There are differences of detail which prevent an accurate identification of phase with phase. And even although all the instruments in use were mechanically identical, the character of the ground in the vicinity of each station could hardly fail to have a modifying influence.

Whatever precision may attend the identification of the most energetic phase of the large waves, we cannot with the same assurance regard the beginnings of the preliminary tremors at the different stations as necessarily belonging to the same phase. The decay of energy in the motion as it reaches to further distant points will tend to obliterate the earlier parts of the preliminary tremors. The instrument will begin to record when the movements are sufficiently large; and thus the first appearance of the preliminary tremors will be retarded in time as the radiation passes out to more distant points. In other words, the speeds of transit to distant stations as determined by the advent of the preliminary tremors must in general be lower than it ought to be when compared to the speeds of transit determined from the records of less distant places. It seems to me that we have interesting illustrations of this retardation in the records of the Indian earthquake of April 4, 1905. Omori has collected the data of this earthquake in an important memoir published in 1907; and as an illustration of the nature of the material at our command I reproduce the principal features of the records of twenty-eight out of the sixty-nine stations at which records were obtained. I have accepted Omori's estimate both of the centre of the shock and of its time of occurrence at the epicentre, although I think his method of using the magnetograph indications at Dehra Dun open to criticism. If we place the epicentre somewhat to the north-west of the position assumed by Omori, and estimate the time of occurrence nearly a minute sooner than he has done, we get

results quite as reasonable on the whole and more in accordance with the curiously delayed record at Kodaikanal. It will be seen in the table that both the first and second phases of the preliminary tremors are much retarded at Kodaikanal as compared with their advent at Calcutta, and yet the distances from the assumed epicentre are only as three to two. C. Michie Smith, the Director of the Observatory at Kodaikanal, informs me that he is sure of his time to a few seconds. Simply to say that the Kodaikanal time is too late does not explain matters. It seems to me that this record should have been given equal weight at least with the magnetograph record of Dehra Dun. The alteration of the epicentre and epoch in the way suggested will not appreciably affect the results except for a few stations near the epicentre such as Calcutta, Bombay, Kodaikanal, and Taschkent. For present purposes, however, Omori's reckonings may be accepted as a good approximation to the truth.

Accordingly, the epicentre from which the tabulated arcual distances of the various stations are measured is assumed to be in latitude 31° 49′ N. and longitude 77° E., and the epoch of the earthquake is taken at 0 h. 49·8 min. G. M. T. The table then gives the times of transit of the preliminary tremors ($P$ representing the first and $S$ the second phase) and certain phases of the large waves, $l$ corresponding to Omori's first phase, $L$ to his third phase, and $M$ to the maximum as it appears on the records of Milne's horizontal pendulums. $L$ marks the beginning of the most active part of the record. The column headed $W$ refers to what is generally accepted as the appearance of the large waves after passing round the earth by way of the longer arc of the great circle containing the epicentre and the station corresponding. It may be compared with $L$, the respective times of transit of $L$ and $W$ being in the ratio of $A°$ to $(360° - A°)$, where $A°$ is the arcual distance of the station. The next column gives the maximum amplitude of each record reduced in the ratio of the multiplication ratio of the instrument; and the next again the ratio of the amplitudes of $L$ and $W$. The last column contains information concerning the types

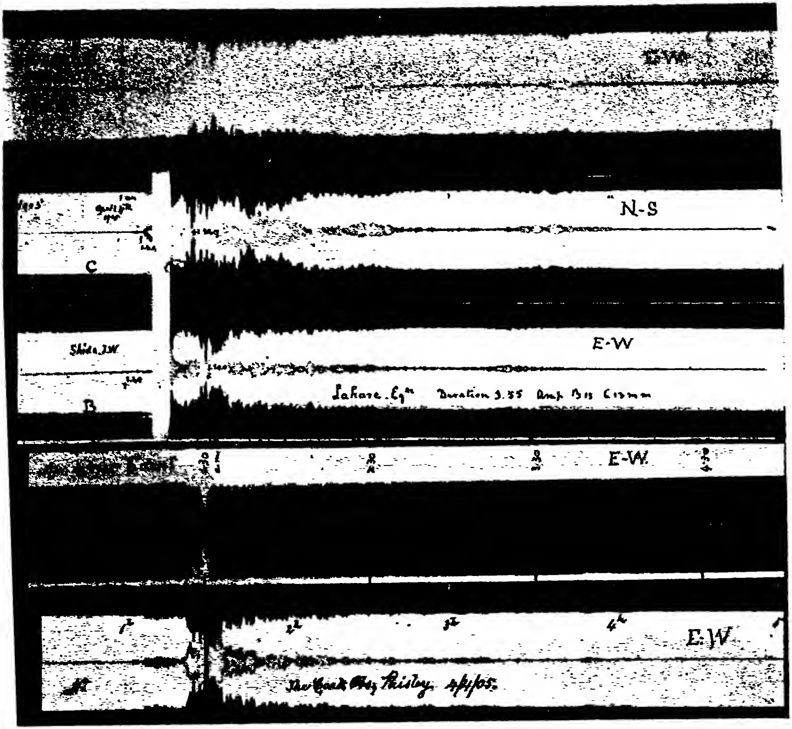



FIG. 40. VARIOUS RECORDS OF KANGRA EARTHQUAKE.

(Reduced in the ratio of 13 to 28.)

of instrument used—the initials referring to the names of the inventors, Rebeur-Ehlert, Milne, Omori, Vincentini, Wiechert, Stiattesi.

SEISMIC RADIATIONS

| Station | Arcual Distance | Transit Times of the Phases | | | | | | Amp. | Instrument |
|---|---|---|---|---|---|---|---|---|---|
| | | *P* | *S* | *l* | *L* | Max. | *W* | | |
| Taschkent . . . | 11° 20′ | 2·6 | 4·7 | — | — | 7·5 | — | 5·1 | R-E |
| Bombay . . . . | 13° 28′ | 3·3 | 6·9 | — | — | — | 267·2 | — | M |
| Calcutta . . . . | 13° 42′ | 2·2 | 5·4 | — | — | — | 251·8 | — | M |
| Kodaikanal . . . | 21° 35′ | 6 | 9·9 | — | — | — | - | — | M |
| Tiflis . . . . . | 27° 26′ | 6 | 10·7 | — | — | - - | - - | 24+ | O |
| Beirut . . . . . | 34° 41′ | 8·2 | — | — | — | 34·7 | — | 1·9 | M |
| Taihoku (Formosa) | 39° 27′ | 7·5 | 13·6 | 19 | 23·5 | — | — | 1·6 | O |
| Manila . . . . | 43° 34′ | 8·6 | 15·3 | — | 26·5 | — | - | 2·7 | V |
| Leipzig . . . . | 50° 16′ | 8·9 | 15·9 | 23·8 | 30·3 | — | — | 1·9 | W |
| Tokyo . . . . . | 51° 26′ | 9·3 | 16·6 | 24·3 | 32·6 | — | 223·8 | 3·1 | O |
| Göttingen . . . | 51° 45′ | 9·1 | 16·4 | 23·9 | 30·5 | — | 235·1 | 3·3 | W |
| Quarto-Castello . | 51° 56′ | 9 | 16·2 | 23·8 | 31·1 | — | — | 2·5 | St |
| Mauritius . . . | 55° 15′ | 9·1 | 17·7 | 26 | — | — | — | — | M |
| Birmingham . . | 58° 49′ | 10·8 | 19·1 | 26·8 | 35·6 | — | 208·5 | 0·8 | O |
| Shide . . . . . | 58° 52′ | 10·8 | 16·2 | — | 32·9 | 40·2 | 228·9 | 1·5 | M |
| Paisley . . . . | 59° 30′ | 10·2 | 18·5 | — | 34·1 | 40·3 | 238·4 | 1·4 | M |
| San Fernando . . | 66° 47′ | 12·7 | 21·9 | — | 37·2 | 44·2 | 228·2 | 0·88 | M |
| Azores . . . . | 79° 54′ | 11·2 | — | — | — | 64·7 | — | 0·23 | M |
| Cape Town . . . | 85° 46′ | 12·7 | 22·7 | — | 46·4 | 56·2 | 198·5 | 0·3 | M |
| Victoria (B. C.) . | 97° 42′ | 17 | 24·2 | 48·2 | 60·5 | 67·3 | — | 0·6 | M |
| Toronto . . . . | 101° 30′ | 16·8 | 24·4 | 48·2 | 60·4 | 64·5 | — | 0·4 | M |
| Washington . . . | 105° 17′ | 18·6 | 33·7 | 45·8 | 61·5 | — | 188·8 | 0·6 | O |
| Cheltenham . . . | 105° 22′ | 18·9 | 33·6 | 45·8 | 61·3 | — | 188·5 | 0·3 | O |
| Honolulu . . . | 108° 45′ | 14·8 | 18·9 | — | — | 65·7 | — | 0·4 | M |
| Samoa . . . . . | 115° 08′ | 11·6 | 29·5 | 50·2 | 60 | — | · | 0·4 | W |
| Christchurch . . | 115° 03′ | 20·2 | — | — | — | — | 157·7 | 0·9 | M |
| Wellington . . . | 115° 45′ | 20 | 31·1 | — | 70·1 | · | 159·5 | 1·3 | M |
| Tacubaya (Mexico) | 128° 39′ | 21·6 | 38·3 | 54 | 72 | · | 178·6 | 0·2 | O |

Reduced copies of the seismograms of the Kangra earthquake obtained at some of these stations have been reproduced in Fig. 40. In Fig. 41 an interesting collection of seismograms obtained by the Milne form of instrument at Paisley has been reproduced by the kind permission of the Directors of the Coats's Observatory.

The above may be taken as a good example of the kind of data the seismologist is called upon to deal with. Glancing down the columns we see that the times of advent of the different phases become on the average later and later the further the station is distant from the origin. There are,

however, discrepancies or irregularities which are occasionally not a little puzzling.

The best way to bring out the average relations and at the same time see where the irregularities are most in evidence is to plot the related numbers on section paper, and draw by freehand an average curve through the points so obtained. Let the arcual distances be measured horizontally and the corresponding values of the transit time vertically. When this is done the various sets of large wave points will be found to cluster round straight lines which pass through the origin (see Fig. 39). This result was obtained by Milne in 1898 (see British Association Report for that year), when sufficient material of this kind was for the first time collated. But, as was also established by Milne in 1898, the preliminary tremor points cannot be grouped about a straight line through the origin. The time graphs for both phases of the preliminary tremors are convex upward, at any rate for the first 90°. If we draw such a curve through the mean positions up to 100°, taking into account both Honolulu and the Canadian stations (as given by Omori), we shall not find it possible to continue the curve so as to pass near the points corresponding to the stations at greater distances. Thus the Samoa time is much too short and the times in the other cases are too long. By neglecting the observations at Ponta Delgado (Azores), Cape Town, and Honolulu, Omori is able to get a fairly continuous curve passing near most of the points. This curve begins convex upwards, but becomes perfectly straight from 60° onwards. When, however, we study the arrangement of the points as a whole, and especially when we bear in mind the evidence derived from other earthquakes, Omori's graphical representation seems to be open to criticism. It is not a sound scientific method to neglect the data supplied by three nearer stations simply because they do not harmonize with what is believed to be the evidence of nine more distant stations. Moreover, a careful study of the seismograms of several of these distant stations as supplied in Omori's valuable memoir shows that the preliminary tremors began in a very uncertain manner.

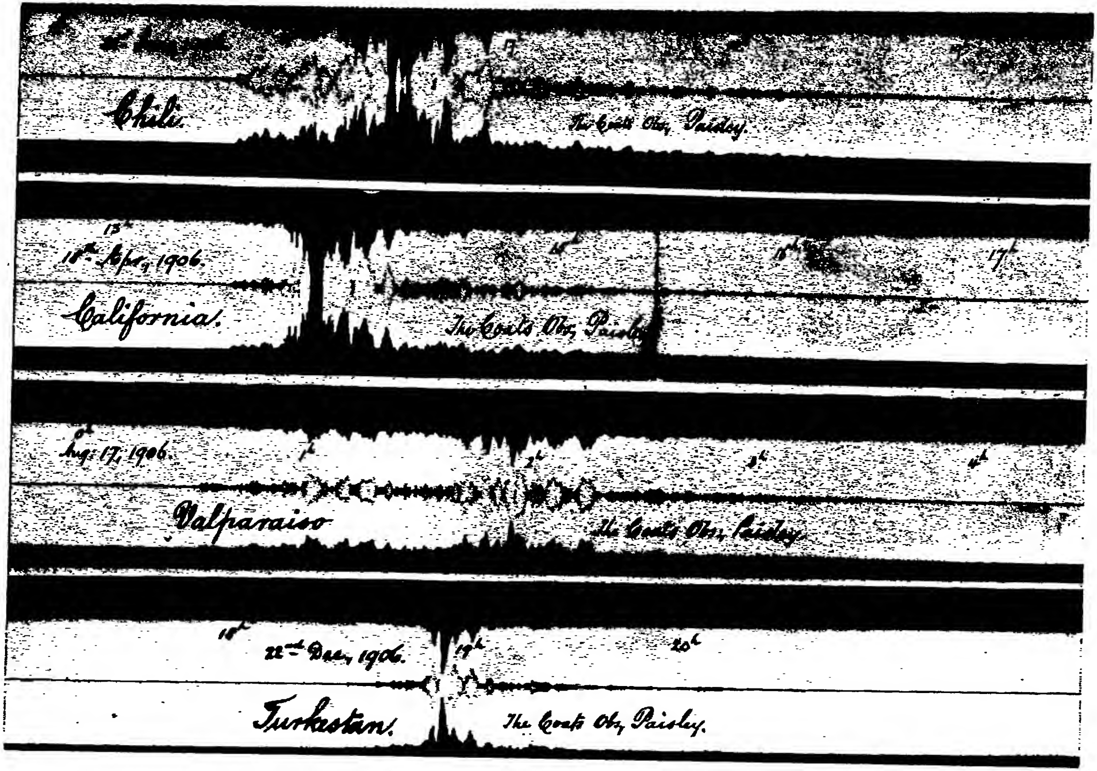



Fig. 41. Paisley Records of Various Earthquakes.

Take, for example, the Canadian stations Toronto and Victoria. The directors of the observatories there marked the times of advent of the first preliminary tremors eight minutes later than the times which Omori himself made out from the records. Probably Omori is right in thus correcting the original measurements. There is a very slight appearance of an earlier movement; but the indications are extremely uncertain and might be easily enough discovered to be some minutes earlier still. Similarly on most of the other records from distant stations there are the same uncertain beginnings. The movement indicated is at the best extremely small, and does not begin in the clear abrupt manner so characteristic of records obtained at stations within 60° distance from the epicentre. We have indeed no assurance that the more distant stations obtain records which in their beginnings have point to point correspondence with the beginnings of the records obtained at nearer places. An earthquake large enough to send its tremors all over the world may not be able to transmit the feebler of these tremors in sufficient intensity to be recorded at distant stations. Several undoubted cases of rapid transmission to stations approximately antipodal to the epicentre will be more valuable evidence in this connexion than many cases of apparently slower transmission.

Some idea of the rate of decay of intensity with distance may be obtained by consideration of the relative amplitudes of the records of the same phase of motion at different distances. Glancing down the column of amplitudes in the table on page 215 we see that in a general way the maximum amplitude diminishes as the distance from the epicentre increases. But the complexities introduced by resonance and the diversities in the character of the soil and in other conditions of installation absolutely prohibit us from making any quantitative comparisons. There is, however, another method to our hand depending upon the fact that with sufficiently strong shocks the surface disturbance which constitutes the large waves can in many cases be recognized after having passed through the antipodes and back

again towards the epicentre. Presumably, if what are symbolized above by the letters *L* and *W* refer to the same type of movement in its radiation over the surface of the earth, similar periods ought to be discoverable in them, producing therefore similar effects of resonance on the instrument at a given station. We have therefore some justification in comparing the maximum amplitudes of *L* and *W* and in deriving from this comparison the law of decay of the surface wave as it spreads out over the surface.

The law of apparent decay will depend on two factors, (1) the loss of energy due to friction and viscosity, (2) the manner in which wave front expands up to the quadrantal distance from the epicentre and contracts thereafter towards the antipodal point. Passing through this point it expands again and finally contracts as it runs in upon the epicentre. But this second factor does not affect the result when we compare the amplitudes of the *L* and *W* disturbances at any one station. From the seismograms published in Omori's report I have constructed the following table giving the ratio of the maximum amplitudes for *L* and *W* at four stations; and it is remarkable how well they agree. The first column of numbers gives the minor arcual distances from the epicentre, and the second column the major arcs, the sum of each pair being 360°. The third column contains the ratio of the amplitudes, and the last column the logarithmic decrement of amplitude calculated per degree of arc.

| Station | Arcual Distance | | Ratio of Amplitudes | Coefficient of Decay |
|---|---|---|---|---|
| | Minor | Major | | |
| Tokyo . . . . | 51°·4 | 308°·6 | 50 | 0·015 |
| Birmingham . . | 58°·8 | 301°·2 | 60 | 0·017 |
| Cheltenham . . | 105°·4 | 254°·6 | 14 | 0·0175 |
| Washington . . | 105°·3 | 254°·6 | 12 | 0·017 |

The calculation is made on the usual assumption that the amplitude falls off according to the exponential law. Thus if *A* and *a* represent the amplitudes respectively of the

$L$ and $W$ disturbances at a station whose arcual distance is $x$ from the epicentre, then the ratio

$$\frac{A}{a} = e^{k(360 - 2x)}$$

where $e$ is the number 2·71828.

With this value of $k$, namely, 0·017, we find that the amplitude of the movement is cut down to half its amount at a distance of 41°·2.

This seems to me to be a fact of considerable importance. We cannot escape from the certainty that similar though possibly smaller rates of decay will rule in the other kinds of motion transmitted. It is consequently not surprising that the first appreciable tremors of the preliminary phase should be retarded in time at certain of the more distant stations. When these first indications come on with an almost imperceptible but gradually increasing movement, we should in most cases be justified in regarding the commencement as uncertain and probably delayed.

This killing out of the early tremors will no doubt be more in evidence in some regions than in others. The nature of the underlying soil and rocks will be a determining factor. Thus there seems to be a tendency for the Canadian stations to give records which have a retarded commencement. My own suspicion is that the times originally given by Mr. Baynes Reid at Victoria (B. C.) and by Professor Stupart at Toronto really refer to the advent of the second phase of the preliminary tremors. Also a careful inspection of the Cheltenham (U. S.) and Washington seismograns shows that the second phase might have begun about five minutes sooner than the times chosen by Omori. Occasionally, indeed, it is not very obvious what considerations have led to the identification of the second phase. The mind seems to be unconsciously led to fix upon the time from a certain expectation as to when it ought to be.

There is again the early arrival of the preliminary tremors at Cape Town, the Azores, and Honolulu. This may be explained as due to pulsations coexisting with the true

seismic vibrations. But it is also conceivable that at these stations the rapid tremors are not so quickly killed out, perhaps because they are in more continuous material touch with the inner substance of the earth.

There are indeed so many factors to be considered that we cannot expect to get all we want from one particular earthquake. We must compare the results of a great number.

As already pointed out, Milne was the first (in 1895) to discuss generally the relations between distance and times of transit of the preliminary tremors and the large waves: and in 1898 (see British Association Report) he showed how origins could be approximately determined from a knowledge of the interval of time by which the large waves were outraced by the preliminary tremors. When we know this difference for two stations we can by means of a little arithmetic and the laying off of two arcs on a globe obtain two points of intersection near one of which the origin will be. Our knowledge of the seismic regions of the globe enables us in general to choose one of these as the epicentre. Indeed this knowledge enables us not unfrequently to infer the whereabouts of the origin from inspection of one seismogram only. Using the results from three stations we get the epicentre at once as the approximate intersection of three arcs.

Omori's method of locating the epicentre is the same in principle. By combining the records of a number of earthquakes he finds that the distance of the epicentre from a station whose record gives $t$ seconds for the duration of the preliminary tremors before the advent of the large waves may be expressed by the formula

$$\text{Distance in kilometres} = 17{\cdot}1t - 1360.$$

Strictly speaking, the formula holds for durations lying between four and eleven minutes, that is between 25° and 90° arcual distance. When the duration of the first preliminary tremors only is taken into account the formula is

$$\text{Distance} = 6{\cdot}54t + 720.$$

Omori has used these formulae in fixing the position of

epicentres not very distant from Tokyo. For more distant earthquakes the linear relation breaks down.

A similar formula has been elaborated by W. Laska,[1] who has also added an obvious but laborious mathematical method for calculating the position of the epicentre from the three distances estimated by means of the formula. It is very doubtful if the accuracy of the data is sufficient to repay the labour of undertaking this calculation. In any case a linear formula applies only within a limited range, whereas Milne's original method takes direct account of the time curves. With these as guide the laying off of arcs on a good sized globe is at present to be preferred.

Milne has systematically applied this method to the determination of earthquake origins since 1900; and beginning with the year 1902 he has prepared for the British Association Report, year by year, a map giving the total number of large earthquakes which have originated since 1899. The map, bringing the statistics down to the end of 1906, is reproduced in chapter vi, p. 97, where also will be found a description of the areas and numbers marked on it.

In the table following I have arranged the main facts contained in Milne's maps, so as to show the total number of large earthquakes which have originated in the several districts since the method began to be applied, and also the annual addition to this total year by year since 1901. It will be noticed that all the districts in which the earthquakes originate are essentially oceanic or littoral, except district K, which runs from the south-east of Europe to the Himalaya mountains. A, B, D lie on the west coast of the Americas, beginning at Alaska; C includes the West Indies; E is the Japan district, and F the East Indies region. G lies in the Indian Ocean; H, I are in the North Atlantic; J is in the Arctic regions, and L in the Antarctic. The district M was introduced to take into account the earthquake tremors which were recorded by the scientific staff of the *Discovery* in her visit to the Antarctic regions in 1902. There is some doubt as to the meaning of the records obtained by the

[1] See *Mitteilungen der Erdbeben-Kommission* of the Vienna Academy of Sciences (1903).

*Discovery*; some may have been due to the shakings of the ice quite apart from real seismic disturbances. They are entered in the table, but are not taken into account in the general discussion.

MILNE'S LIST OF LARGE WORLD-SHAKING EARTHQUAKES

| | Totals since 1899 to | | | | | | Number each Year | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1901 | 1902 | 1903 | 1904 | 1905 | 1906 | 1902 | 1903 | 1904 | 1905 | 1906 |
| A | 25 | 27 | 30 | 30 | 32 | 34 | 2 | 3 | 0 | 2 | 2 |
| B | 14 | 22 | 28 | 28 | 32 | 36 | 8 | 6 | — | 4 | 4 |
| C | 16 | 22 | 25 | 25 | 25 | 29 | 6 | 3 | — | — | 4 |
| D | 12 | 15 | 16 | 16 | 16 | 20 | 3 | 1 | — | — | 4 |
| E | 29 | 38 | 45 | 59 | 67 | 85 | 9 | 7 | 14 | 8 | 18 |
| F | 41 | 55 | 66 | 75 | 94 | 115 | 14 | 11 | 9 | 19 | 21 |
| G | 17 | 18 | 21 | 21 | 22 | 23 | 1 | 3 | — | 1 | 1 |
| H | 22 | 22 | 24 | 25 | 28 | 31 | — | 2 | 1 | 3 | 3 |
| I | 3 | 3 | 5 | 5 | 5 | 5 | — | 2 | — | — | — |
| J | 3 | 3 | 3 | 3 | 3 | 3 | — | — | — | — | — |
| K | 14 | 36 | 58 | 62 | 77 | 91 | 22 | 22 | 4 | 15 | 14 |
| L | 2 | 2 | 2 | 2 | 2 | 2 | — | — | — | — | — |
| M | | — | — | (75) | (75) | (75) | — | — | — | — | — |
| | 198 | 263 | 323 | 351 | 403 | 474 | 65 | 60 | 28 | 52 | 71 |
| Annual Average | 66 | 65·8 | 64·6 | 58·5 | 57·6 | 59·3 | | | | | |

The most active districts are E, F, and K, which lie close to and within the great Asiatic continent. The North American districts A, B, and C are more energetic seismically than the South American district D. Clearly, extent of stretch of continent has an important bearing on seismicity as well as height of land above ocean depths. The earthquakes originating in H were comparatively small, although in number they exceed those of D.

There is a marked minimum activity in the year 1904. The records do not go quite far enough back to indicate whether there was a maximum in 1900 or 1901. The averages hint at the existence of a maximum in either of these years. The year 1901 was a year of minimum sunspot activity. It would be curious indeed if minimum solar activity should be accompanied with maximum terrestrial seismic activity.

There is no doubt that the year 1906 impressed the imagination as a year of special activity. This was because several of the world-shaking earthquakes occurred in civilized and densely populated regions. It will be noted, however, that the districts B, C, and D, within which San Francisco, Kingston, and Valparaiso respectively lie, were not particularly active; the great increase in the number throughout the year 1906 occurred in districts E and F, especially in the former. The origins must have been well out to sea, for Japan itself was not visited by any destructive seismic disturbance.

District G is credited only with one earthquake origin in 1906. This district, indeed, which in 1901 appeared to be as important a seismic region as district K, has steadily fallen behind during the succeeding years. It is possible that the early statistics utilized by Milne were not so accurate as the later ones, and that we may have to consider district G as of relatively small importance.

With the accumulation of statistics year after year, the earlier results obtained by Milne have been in the main corroborated. In 1900 Oldham [1] discussed the data of seven large earthquakes, and drew the time-curves for the first and second preliminary tremors, and also for the commencement and maximum of the large wave portion. With still fuller statistics Milne in the British Association Reports for 1902 gave average time-curves which are shown in Fig. 42. Each dot or cross represents one result from a particular earthquake.

The arcual distances of the stations from the seismic focus in each earthquake for which records were obtained are measured horizontally, and the corresponding times of the respective motions reckoned from the epoch are measured vertically. The points so obtained cluster along and around three lines. In the case of the large wave maximums the line is practically straight, showing that this maximum phase in all probability passes through the earth's crust close to the surface, with an average speed of 95° per hour,

[1] 'Propagation of Earthquake Motion to Great Distances,' *Philosophical Transactions*, vol. 194.

or 1°·6 per minute. If we take the averages of the various rates of transit in the case of the Kangra earthquake we find that the first phase, *l*, the third phase, *L*, and the maximum, *M*, passed through the crust close to the surface

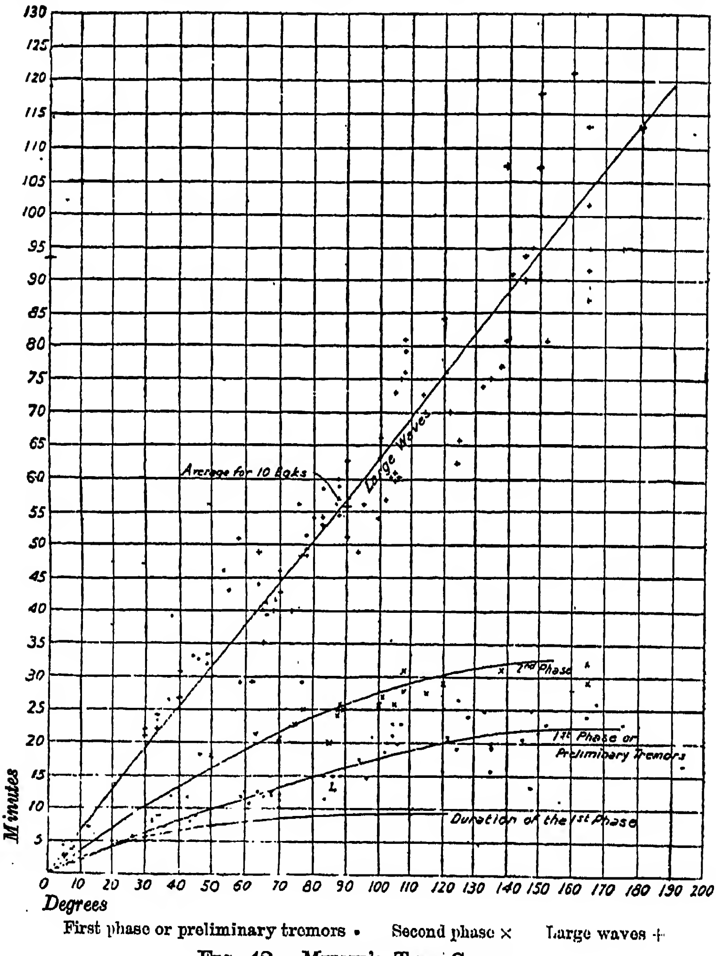



FIG. 42. MILNE'S TIME-CURVES.

with the respective speeds of 2°·18, 1°·71, and 1°·46 per minute. It thus appears that the large wave seismic radiations of the Kangra earthquake passed a little more slowly through the crust than the average for the cases considered by Milne.

The lines representing the average time-curves for the first and second phases of the preliminary tremors are distinctly not straight. The curves seem to tend towards a maximum as they approach the arcual distance of 180°.

These waves pass through the earth by brachistochronic paths, that is, paths of shortest time, the form of which depends on the manner in which density and elasticity vary with depth below the surface.

In the British Association Report for 1902 I pointed out that the form of Milne's time-curves at once suggested a very approximate chordal transmission. This is brought out in the following table, which is a modification of that originally given in the Report. The first column gives the arcual distances, the second and third the average times of start of the two phases of the preliminary tremors, the fourth and fifth the chords and arcs in terms of the earth's radius as unity, and the remaining columns the corresponding chordal and arcual speeds of the two phases in earth-radius per minute. This mode of expressing the speed has the convenience that the same unit is employed in each. We can easily pass to miles per second or kilometres per second by multiplying respectively by 66 and 106.

Chordal and Arcual Speeds of P and S phases of Preliminary Tremors in Earth-Radius per Minute

| Arc | Times | | Chord | Arc | Chordal Speeds | | Arcual Speeds | |
|---|---|---|---|---|---|---|---|---|
| | P | S | | | P | S | P | S |
| 20° | 4 | 6·9 | 0·34 | 0·35 | 0·087 | 0·050 | 0·087 | 0·051 |
| 40° | 8 | 13·3 | 0·68 | 0·70 | 0·086 | 0·051 | 0·087 | 0·052 |
| 60° | 11·4 | 19 | 1 | 1·05 | 0·088 | 0·053 | 0·092 | 0·055 |
| 80° | 15 | 23·0 | 1·29 | 1·40 | 0·086 | 0·054 | 0·093 | 0·058 |
| 100° | 17·7 | 27·8 | 1·53 | 1·74 | 0·086 | 0·055 | 0·099 | 0·063 |
| 120° | 20 | 30·3 | 1·73 | 2·09 | 0·087 | 0·057 | 0·105 | 0·069 |
| 140° | 21·7 | 32·1 | 1·88 | 2·44 | 0·087 | 0·059 | 0·113 | 0·076 |
| 160° | 22·2 | 32·9 | 1·93 | 2·79 | 0·087 | 0·059 | 0·126 | 0·085 |

These numbers indicate that the transit speeds of the preliminary tremors measured as along the arc increase with the distance. Whatever may be the form of the brachistochronic paths which all such vibratory motions necessarily

pursue, they certainly do not confine themselves to the crust through which the large waves are propagated. The remarkable constancy of the speed of the first preliminary tremors, estimated as if transmitted along the chord, suggests that the brachistochronic paths cannot deviate far from the chords through the greater part of their course. The same constancy does not characterize the second preliminary tremors. A comparison of their arcual and chordal speeds shows, however, that the paths come nearer to the chordal position than to the arcual.

The broad conclusions which may be drawn from these earlier results are that the large waves are propagated round the immediate interior of the earth's crust, like sound round a whispering gallery, with a speed of 1·85 miles or 3 kilometres per second; that the first phase of the tremors is transmitted across the earth's diameter with a speed of 5·7 miles or 9·17 kilometres per second; and that the second phase passes across the same diameter with a speed of 3·9 miles or 6·25 kilometres per second.

Some of the conclusions just indicated are not accepted by the Japanese seismologists. Omori considers that all types of wave are transmitted arcually, not necessarily along the surface, but along appropriate arcual layers parallel to the surface. The general idea is that there is a speed peculiar to each layer, and that there is a particular layer of maximum velocity along which the preliminary tremors are transmitted. In No. 16 (1904) of the Publications of the (Japanese) Earthquake Investigation Committee, Imamura has brought together a large amount of material in support of this view. No fewer than eighty-five earthquakes are discussed; but in regard to the debated question of the transit speed of the preliminary tremors, the great majority of these are, on the author's own admission, of absolutely no account. The origins of only fifteen of them were definitely known; in all other cases the origins were estimated by comparisons of the seismograms at different stations. This method is sufficiently satisfactory for ordinary purposes; but it can hardly be accepted as a trustworthy basis for a sound scientific argument as to the

speeds of propagation of the various phases of the earthquake motion. But a much more serious objection to Imamura's method is that in all but three or four cases he has estimated the epoch of the earthquake at the origin on the assumption of *what he wants to prove*, namely, the arcual propagation of the preliminary tremors. Indeed, it cannot be too strongly urged that those earthquakes only whose epochs are known definitely to within a minute are of any value in an investigation of this kind. It is not always possible to get this information, especially if the origin is under the ocean. Even in cases where the origin is under the land there is frequently difficulty in timing the exact occurrence of the quake, chiefly because of the lack of accuracy in the measurement of local time.

The most complete of all the cases discussed by Imamura is the Guatemala earthquake of 1902. Oldham [1] has criticized the methods and conclusions of the Japanese seismologist, and has brought together all the possible material available for a complete study of the transit velocities of the first and second tremors and of the large waves. He shows that Milne's original estimate of the epoch (2 h. 22 m. G.M.T.) is more accurate than that adopted by Imamura, namely, 2 h. 26 m. This difference of four minutes will affect considerably the estimated speed of propagation, and will quite destroy the linearity of the time graph of the first tremors regarded as a whole. Forty-six of the sixty-one individual records collected by Oldham are grouped together according to distance in seven average groups. Dividing the arcual distance of each average group from the origin by the corresponding time reckoned from the epoch, Oldham finds that this ratio increases with the arcual distance.

I have discussed the data in a different way. Assuming a linear relation between the time and the distance, arcual or chordal as the case may be, I have worked out, by the method of least squares, the coefficient of each term. Calling the arc $\theta$, and the chord $x$, and representing by $t_1$ and $t_2$ the times respectively of the first tremors and the large wave

[1] 'The Great Guatemala Earthquake,' *Proceedings of Royal Society*, 1905.

maximum reckoned from the epoch of the earthquake, I find the relations to be as follows :—

| First Tremor | Large Waves' Maximum |
|---|---|
| $\theta = 0{\cdot}319 + 0{\cdot}134t_1$ | $\theta = 0{\cdot}0066 + 0{\cdot}0302t_2$ |
| $x = 0{\cdot}097 + 0{\cdot}0976t_1$ | $x = 0{\cdot}3062 + 0{\cdot}0185t_2$. |

The first term in each expression shows to what degree of approximation the times and distances can be regarded as strictly proportional. The smaller this term the better the approximation. Evidently the chord formula is better for the first tremors, and the arc formula for the large waves. There is no question as to the mode of propagation of the large waves; the chord or $x$ formula is meaningless, demanding an epoch about seventeen minutes earlier than the true epoch. But it is the chord formula which best suits the first tremors, the arc or $\theta$ formula demanding an epoch 2·4 minutes later than the true epoch.

The coefficient of $t$ in each represents the speed of propagation in earth-radius per minute. Hence the first tremors are propagated with the average chordal speed of 6·4 miles per second or 10·3 kilometres per second; while the large waves' maximum is propagated with an arcual speed of 2 miles per second, or 3·2 kilometres per second.

In a recent paper on the Constitution of the Interior of the Earth[1] Oldham has considered with great care the returns for twelve earthquakes, for most of which the origins and epochs were known with fair accuracy. Six of these originated in or near Japan, and the others in Argentina, India, Turkestan, the Philippines, Guatemala, and Kashgar. Unfortunately four of the Japanese earthquakes had their origins well out to sea, so that their positions were matter of inference only. From these inferred distances which might easily be in error by 50 to 100 kilometres the epochs were obtained on the assumption that the disturbance which was felt in Japan travelled at the rate of 3 kilometres per second. This, however, is at best an average; and even if the time observations themselves were above criticism this way of estimating may easily cause an error of half a minute

[1] *Quarterly Journal Geological Society*, 1906.

or more. But in the discussion of the transit speed of the first preliminary tremors the great majority of the time intervals which fall to be considered vary from six to fifteen minutes. In such a case an error of half a minute is proportionately large.

There can be no doubt, however, that Oldham has made the most of the material to hand, and that his results may be accepted as the best averages so far obtained.

I propose now to embody in one comparative table the times of transit for various distances as given by Milne, Oldham, and by the returns for the recent earthquakes of Kangra and San Francisco. I have also added the averages prepared by G. B. Rizzo,[1] who has charge of the seismological observatory at Messina. These occupy the third column of times of transit as given in the following table. The first three columns of average times are headed by the initials of the authorities named. The numbers for the Kangra (K) and Californian (C) earthquake are those compiled by Omori.

TIMES OF TRANSIT IN MINUTES

| Arc | First Preliminary Tremors | | | | | Second Preliminary Tremors | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | M | O | R | K | C | M | O | R | K | C |
| 20° | 4 | 4·2 | 4·5 | 4·6 | 4·3 | 6·9 | 7·4 | 8·5 | 8·5 | 8·8 |
| 30° | 6·5 | 6 | 6·1 | 6·4 | 6·3 | 10 | 11 | 11·2 | 11·3 | 11·8 |
| 40° | 8 | 7·9 | 7·4 | 7·9 | 8·1 | 13·3 | 13·7 | 14 | 13·9 | 14·5 |
| 60° | 11·5 | 11 | 9·7 | 10·5 | 11·5 | 19 | 19 | 18·6 | 18·9 | 19·2 |
| 80° | 15 | 14 | 11·9 | 13 | 12·9 | 23·9 | 23·2 | 23·8 | 24·7 | 26·2 |
| 90° | 16·2 | 15 | 13 | 14 | 14·3 | 26 | 24·7 | 26·4 | 27·5 | 24·9 |
| 100° | 17·7 | 16·3 | 14·1 | {15·2, 18} | 16 | 27·8 | 26·4 | 31·4 | 30·6 | 26·5 |
| 120° | 20 | 18 | 16·3 | 20·5 | 19 ? | 30·3 | 29 | 33·6 | 36·2 | 32 ? |
| 140° | 21·7 | 20 | 18·6 | — | — | 32·1 | {31·6, 43·2} | 38·6 | — | — |
| 150° | 22·2 | 21 | 19·7 | — | — | 32·7 | 45 | 41·8 | — | — |
| 160° | 22·2 | 21·3 | 20·8 | — | — | 32·9 | 46·3 | 44·8 | — | — |
| 180° | — | 22 | 23 | — | — | — | 50 | 49·5 | — | — |

In the British Association Report for 1903 Milne gave reasons for a considerable correction to be applied to these

[1] *Sulla Propagazione dei Terremoti*, Accad. Reale d. Scienze di Torino, (1907).

times of transit. This will be discussed later. Meanwhile we take the data as they are given in the table.

Rizzo's times of transit increase regularly by 1·1 minute for every 10° increase of distance from 60° upward. This regularity does not appear in any of the other columns, whether these refer to averages or single earthquakes. Taking the numbers as they stand we observe that, on the whole, there is a tendency for the later data to indicate, in the case of the first phase, a time of transit distinctly shorter for the moderately distant stations than was indicated by the earlier data discussed by Milne and Oldham. This is specially marked in the neighbourhood of the quadrantal distance. At the same time there is no such indication in the case of the second phase. The most obvious explanation of this is the increasing number of stations equipped with the most delicate types of seismometers. It is in Europe mainly that this great development has taken place; and Europe happens to be just about the quadrantal distance, more or less, from many centres of world-shaking earthquakes. With more sensitive instruments the first beginnings of the earthquake tremors will naturally be observed sooner. But once the tremors are sensible on the seismometer record the advent of the second phase will not be so influenced by the sensitiveness of the instrument.

The times of transit for the distance 180° cannot be trusted, inasmuch as they have been obtained by a process of extrapolation. There is only one case I know of in which a distinct record was obtained at a distance approaching 180°. This was the Caracas earthquake of October 29, 1900. Batavia, according to Imamura's list, was distant 175° from the epicentre; but I have no information regarding the epoch of the shock. We find, however, that the record began at 9 h. 32·4 m. at Batavia, and at 9 h. 20·9 m. at the distance of 60° (mean of two). Taking 10·5 as a fair mean value of the time of transit to the latter distance, we find for the probable time of transit to a distance of 175° the value 22 minutes. This agrees with Oldham's estimate for 180°, and may be taken as the best uncorrected value obtainable.

The second phase is very difficult to identify in seismo-

grams obtained at distant stations; and it is doubtful if we are in a position to say anything definite as to the time of transit for distances greater than 150°.

Taking all circumstances into consideration, we may accept Oldham's average values for the first phase as the most trustworthy. If following the lead of the Kangra and San Francisco earthquakes we decrease the times of transit to approximately quadrantal distances, but adhere to Oldham's values for greater distances, we get results which at once suggest retardation of the first appearance of the tremors. On forming the time graph after the manner of Milne's chart, we find that the curve giving time in terms of arcual distance begins convex upward, then becomes flattened out or even concave upward in the neighbourhood of 90°, and finally finishes with an upward convexity. Oldham's curve is convex all the way, with a slight break in the continuity of curvature at about 140°.

I now reproduce Oldham's condensed table of averages, treated in the manner in which I have treated Milne's results above (p. 225).

OLDHAM'S TIMES OF TRANSIT OF EARTHQUAKE TREMORS AVERAGED FROM TWELVE EARTHQUAKES. SPEEDS IN EARTH-RADIUS PER MINUTE

| Arc | Times | | Chord | Arc | Chordal Speeds | | Arcual Speeds | |
|---|---|---|---|---|---|---|---|---|
| | P | S | | | P | S | P | S |
| 20° | 6 | 11 | 0·518 | 0·524 | 0·086 | 0·047 | 0·087 | 0·048 |
| 60° | 11 | 19 | 1·00 | 1·05 | 0·091 | 0·053 | 0·095 | 0·055 |
| 90° | 15 | 25 | 1·41 | 1·57 | 0·094 | 0·057 | 0·105 | 0·063 |
| 120° | 18 | 29 | 1·73 | 2·09 | 0·096 | 0·060 | 0·116 | 0·072 |
| 150° | 21 | 45 | 1·93 | 2·62 | 0·092 | 0·043 | 0·125 | 0·058 |
| 180° | 22 | 50 | 2·00 | 3·14 | 0·091 | 0·040 | 0·143 | 0·063 |

The chordal speeds are not so steady in value as in the case based on Milne's averages; but indeed the values obtained from Milne's statistics were too regular to satisfy a true chordal transmission. For we know that the speed of transmission is certainly less in the upper than in the lower parts of the crust. In coming out to the surface the ray of

disturbance must pass through these upper layers. The average speed is all that we can get from the data; and since proportionately more of the upper crust is passed through the less distant the observing station, it is clear that the average speed will fall short of the speed in the deeper regions by an amount which will be greater for the nearer station. Thus the increase in the chordal speeds as the distance increases up to 90° is what we should expect for the case of an approximately chordal transmission. After 120° there is a distinct fall off in the average chordal speed. I believe this fall off to be due to high emergence angles and to energy distribution as explained below (pp. 249, 255).

Up to 120° Oldham's delineation of the time graph of the second phase does not differ essentially from his earlier delineation or from Milne's representation given above. Milne gives three points above 120° distance, and these imply a continuation of the curves roughly resembling the form of the graph for the first phase. Oldham, however, gives eight points on the graph at distances greater than 130°; and these lie so much higher than the points for smaller distances that it is not possible to draw a continuous curve through them all. This peculiarity is indicated in the table by the abrupt fall off in the estimated average speed of transmission of the second phase tremors for the two highest distances tabulated. Assuming this discontinuity to be a real effect Oldham proceeds to explain it on the hypothesis of an inner core of the earth transmitting vibrations at a distinctly slower rate than the surrounding shell. His argument is as follows :—

'As regards the size of the core, we have seen that it is not penetrated by the wave-paths which emerge at 120°; and the great decrease at 150° shows that the wave-paths emerging at this distance have penetrated deeply into it . . . Now the chord of 120° reaches a maximum depth from the surface of half the radius . . . so that it may be taken that the central core does not extend beyond about 0·4 of the radius from the centre.'

His conclusion, which may be easily verified, is that the rate of transmission through the core is just about half that

through the shell. This ingenious hypothesis requires that the seismic rays on entering the core will suffer considerable refraction, very similar to but more marked than the refraction of rays of light through a spherical lens of glass or crystal. The law of refraction depends on the ratio of the speeds of propagation in the two media, and the speed of propagation depends (see chapter viii) on the square root of the ratio of the elastic modulus to the density. But it will be noticed that the time graph for the first tremor phase does not show the same discontinuity. There is a slight sag in Oldham's curve as drawn, but it is too slight in itself to serve as basis for a serious argument. Why then should the one set of disturbances be powerfully influenced by the assumed central core, and not the other set? An explanation may, of course, be given on two further assumptions. First, the elastic modulus which determines the propagation of the first phase may vary very nearly proportionately to the density for all parts of the earth; secondly, the elastic modulus on which the transmission of the second phase depends may, on the other hand, vary with depth below the earth's surface according to a law quite different from that which holds for the density changes. Now the densities are the same for both types of disturbance. Hence the marked difference in the behaviour of the two phases must be ascribed to the manner of change of the elastic moduli; and we are driven to the extremely improbable conclusion that one elastic modulus changes slowly and continuously with depth, while the other becomes suddenly reduced to one quarter of its value in the outer shell. Considering the great difficulty in many cases of distinguishing the exact advent of the second phase in seismograms of small intensity, we can hardly regard Oldham's hypothesis as at all convincing until a great many more observations are to hand. In this discussion it does not seem to me that due attention has been paid to the distribution of the energy of the radiations as they are transmitted outward from the earthquake focus.

The data on which Oldham bases his ingenious hypothesis of the inner nucleus of the earth are, indeed, altogether too

meagre. We are hardly yet in a position to construct such a definite picture. With regard to the probable nature of the second preliminary tremor as distinguished from the first, the simplest idea undoubtedly is that we have to do with two types of waves whose speeds of propagation across the substance of the earth are different. If we assume that the earth as a whole behaves like an elastic solid these radiations of seismic disturbance may be compared to the compressional and distortional waves discussed in chapter x. The recognition of the two types has indeed been regarded as a strong argument in favour of the solidity of the earth. But if, as many still believe, the earth is essentially fluid throughout, how are the two types of wave to be explained ?

The Rev. Osmond Fisher, whose treatise on the Physics of the Earth's Crust presents in a masterly manner the argument in favour of the fluidity of the earth, has shown how the difficulty may be met.[1] His fundamental conception is that beneath the solid crust of the earth, estimated to be some eighteen or twenty miles thick, there is a molten magma containing gas in solution. Henry's law of the absorption of gases by liquids is assumed to hold, namely, that the mass of gas dissolved is proportional to the pressure. Since at ordinary temperatures and pressures gases obey Boyle's law, Henry's law may for such cases be stated thus :—The volume of a gas which may be absorbed by a given volume of the liquid is independent of the pressure. This is the form in which Fisher applies it to the case of molten rocks under pressures of several thousands of atmospheres. On this insecure foundation there is developed what seems to me to be an argument so precarious that I dare not venture to present it except in the author's own words.

These I shall give side by side with some critical queries to indicate where the argument seems to be faulty. After the enunciation of Henry's law we read :—

[1] 'On the Transmission of Earthquake Waves through the Earth,' *Proceedings of Cambridge Philosophical Society*, vol. xii, 1904.

'Thus if $rV$ be the volume of gas which can be held in solution by the volume $V$ of the liquid, $rV$ is the same for all pressures. It apparently follows that if the liquid be in a state of compression, so that when the pressure is relieved the volume $V$ expands to $V+v$, the volume of gas which it can then hold in solution will be $r(V+v)$. Consequently an additional volume of gas $rv$, proportional to the increase of volume of the liquid, can be held in solution, and no gas will be extruded in consequence of the relief of pressure until the limit of expansibility of the liquid is reached, after which gas will be extruded as the pressure continues to fall.'

Is there not some confusion here as to the precise interpretation of Henry's law? The volumetric enunciation of the law is a mode of expression based on an experiment which takes no account of the negligibly slight volume changes of the liquid. Have we any warrant to assume that in the initial stages of pressure relief the expansion of the liquid increases its absorptive power for a gas to such an extent as to overbalance the diminution in the mass of gas dissolved which, according to the exact statement of Henry's law, must accompany the diminution of the pressure? Indeed, have we any warrant for the belief that the *elastic* expansion or contraction of a liquid has any measurable influence on its absorptive power for gases?

There then follows a mathematical investigation into the propagation of the complex disturbance here indicated, a disturbance consisting partly of elastic compression and partly of gas extrusion. The result gives a speed of propagation whose square is $Pe/(DP+Der)$, where $D$ is the density of the fluid, $e$ its incompressibility, $P$ the pressure, and $r$ as above. If there is no extrusion of gas this becomes the usual $e/D$. If the liquid were incompressible the disturbance would be transmitted because of the extruded gas with a speed equal to $\sqrt{(P/Dr)}$.

'These two effects cannot be simultaneous because . . . so long as the liquid expands no gas will be extruded.

'Consider then the effect

This paragraph reiterates the succession of changes criticized above, and emphasizes the assumption that the magma must be satur-

of a diminution of pressure upon the magma at the origin of disturbance. The relaxation of pressure, although impulsive, cannot be instantaneous. The magma being under the compression corresponding to saturation, the relief of compression up to a certain point will in the first place cause voluminal expansion, which, as already shown, will not be accompanied by extrusion of gas. This expansion will be propagated as an elastic wave with the velocity $\sqrt{e/D}$. As the relief of compression continues, the expansion of the liquid magma will reach the limit of voluminal expansion and gas will begin to be extruded. This stage of the disturbance will be propagated as a second gaseous wave with a velocity $\sqrt{P/Dr}$ less than that of the elastic wave with extrusion of gas but without further expansion of the liquid element of the magma . . .

'Similar changes will occur at every place which the disturbance reaches during its passage. Thus the first effect will be voluminal expansion, so that an elastic wave will be continually started in front of the gaseous wave.'

ated, the necessity for which on other grounds is not apparent.

Still, admitting the delicate adjustment and the changes which are believed to occur, let us consider on its own merits the mode of propagation of the so-called gaseous wave. There is, of course, no difficulty about the other.

A succession of pressure changes produces at a certain stage an extrusion of gas, and this phenomenon is propagated through the magma. This implies not only that the whole magma is in its saturated state, but that the necessary pressure changes continue sufficiently energetic and sufficiently prolonged to give rise to the extrusion at every point visited. But any tridimensional radiation of energy means a falling off in intensity as the disturbance reaches out to greater distances. How under such circumstances are we to imagine the persistence of the conditions favourable for the extrusion of the gas ?

Assuming that this curious gaseous wave corresponds to the second preliminary tremors, Fisher proceeds to calculate the quantity $r$. The value comes out 0·0125, the density of the magma being taken at 2·68. Rise of temperature greatly reduces the power of liquids to absorb gases ;

and at the high temperatures which exist in this case we should, reasoning from analogy, expect the absorption to be practically *nil*, unless the high pressures involved have an influence. But this would contradict Henry's law which is assumed throughout. It seems to me indeed that the theory advanced by Mr. Fisher is too ingenious ; but the author himself regards it as having served its purpose if it relieves the anxiety of those who hold by the internal fluidity of the earth.

But may not the facts be more easily taken into account ? It will be granted by all that the physical condition of molten rock under the high pressures that certainly exist in the heart of the earth must be very different from what our surface experience associates with the term fluidity. For example, air at the temperature and pressure which exist at a depth of twenty miles will have a density comparable with that of the rock. Whatever may be the results of individual experiments on the changes of volume when a substance solidifies or liquefies, the probability is that under great pressures and at high temperatures there is no clear line of division or demarcation between the two states. We know from experiment that elastic solids can be made to flow. During this process of yielding to the appropriate stress the material will still behave like an elastic solid to rapid variations of compressional and distortional stresses and even of this very stress which is compelling it to flow. Fluidity and elasticity of form are not of necessity incompatible or mutually antagonistic. There is nothing physically unsound in the hypothesis that the nucleus of the earth is capable of transmitting distortional as well as compressional vibrations, but is at the same time incapable of resisting indefinitely continued action of steady distortional stresses.

If, as is generally admitted, the two phases of the preliminary tremor be comparable with the compressional and distortional waves in an isotropic elastic solid, should we not expect to find some evidence that the vibrations are in the one case longitudinal, in the other transverse ? In other words, by careful comparison of the records of two

seismometers set so as to record movements at right angles to each other, is it possible to detect a difference corresponding to the difference between longitudinal and transverse displacements? With this object in view Imamura[1] has studied the North-South and East-West records of various earthquakes as given by some of the Tokyo instruments. No very clear conclusion can, however, be drawn.

Omori, in his memoir on the Kangra quake, gives results agreeing fairly well with the hypothesis that the first and second preliminary phases are comparable with waves of longitudinal and transverse type. He considers that all the types of motion are transmitted along the arc; but any probable brachistochronic path through the earth will satisfy the surface conditions quite as well. The Japan and European stations are all situated relative to Kangra in such a position that the great circle drawn through each will pass very nearly east and west through the corresponding station. Hence we may assume that the East-West recording instrument will record mainly longitudinal displacements; and the North-South transverse displacements. On the other hand, the American and Mexican stations are so situated that the great circles passing through them and the epicentre are approximately coincident with the meridian. Hence the North-South instrument will record mainly longitudinal vibrations and the East-West transverse. The general results are indicated in the following scheme, in which under each phase is given the azimuth of the instrument which gave the greater movement. Thus N means that the amplitude of the record of the corresponding phase was greater on the North-South instrument than on the East-West; and the letter E means that the East-West record was the larger. The letters heading the columns refer to the different phases, P and S being the preliminary tremors, $L_1$, $L_2$, $L_3$ being Omori's first, second, and third phases of the large waves (p. 200), and W the large waves along the major arc.

[1] *Bulletin of the Imperial Earthquakes Investigation Committee*, vol. i, No. 3, 1907.

PHASE AND DIRECTION OF MOTION

| Region | P | S | $L_1$ | $L_2$ | $L_3$ | W |
|---|---|---|---|---|---|---|
| Tokyo . . . / Osaka . . . / Kobe. . . . | E | N | N | N | E | E |
| Potsdam . . / Gottingen . . | E | N | N | N | E | E |
| Quarto-Castello / Leipzig . . . / O'Gynalla . . | E | N | N | N | N | — |
| Ischia . . . . | E | N | N | N | E | — |
| Tacubaya . . . | N | E | E | E | N | N |
| Cheltenham . . | uncertain | | | | N | — |

Thus there is evidence that longitudinal displacements preponderate in the first preliminary phase and the third phase of the large waves, and the transverse displacements in the second phase and in earlier phases of the large waves.

Rizzo has drawn similar conclusions from the records obtained in Europe of the Calabrian earthquake of September 8, 1905. The four stations discussed are situated to the north of Calabria, so that we may regard north and south displacements as corresponding to the longitudinal vibration.

PHASE AND DIRECTION OF MOTION

| Station | P | S |
|---|---|---|
| Lipsia . . . | N | E |
| Gottingen . | N | E |
| Upsala . . | N | E |
| Tortosa . . | E | N |

Finally, Professor C. F. Marvin, who has charge of the Bosch-Omori seismometers at Washington, and who has improved the action of these instruments by some ingenious devices,[1] found a striking illustration of the same effect in the North-South and East-West records of the Kingston (Jamaica) earthquake of January 14, 1907.[2] Washington is only fifteen minutes of arc west of Kingston at a distance of 1400 miles, that is, almost due north; and the East-West record showed no first phase of tremor. The following are

[1] *Monthly Weather Review*, May, 1906. [2] Ibid., January, 1907.

the times of commencement of the different phases reckoning in minutes from an estimated epoch:—

PHASES AND DIRECTIONS OF MOTION

| | N-S | E-W |
|---|---|---|
| First Phase, Tremors . . | 5·2 | absent |
| Second ,, ,, . . | 9·7 | 9·7 |
| Beginning, Large Waves . | 13·6 | 13·5 |
| End, ,, ,, . | 21·9 | 21·9 |

The earlier advent of the large waves on the East-West record indicates predominant transverse vibrations in the early part of the principal portion. The same result is indicated in Omori's table given above. It has in my opinion a very significant bearing upon the mode of propagation of the large waves; but this is more appropriately discussed further on. (See below, page 257.)

The existence of three distinct types of earthquake radiations is now generally admitted, the most energetic part known as the large waves or principal portion being almost certainly transmitted through the comparatively thin heterogeneous layer which forms the so-called crust of the earth. The first and second phases of the preliminary tremors must be regarded as essentially elastic vibrations transmitted from the earthquake focus along brachistochronic paths to all parts of the earth's surface. My own opinion is that these paths are concave outward but nearly rectilinear in their deeper portions, so that in most cases they do not deviate far from lines of chords.

As already stated, this view is not held by the Japanese seismologists. They favour an hypothesis which explains the times of advent of the preliminary tremors at distant stations in terms of assumed layers of quickest transmission. These layers are believed to lie parallel to the surface, so that the preliminary tremors travel arcually at a depth of perhaps sixty or one hundred miles.

The hypothesis of a stratum of maximum velocity of transmission was first propounded by Nagaoka in his paper of 1900 on the Elastic Constants of Rocks (p. 162). We had long been familiar on seismological grounds with the idea

that the elastic modulus increased with depth, and Nagaoka's experiments on the elastic constants of rocks showed that this increase began to declare itself within the range of the accessible parts of the crust. In his general discussion of this question Nagaoka seems to me to take a curiously limited point of view. I quote the paragraph in which the hypothesis is broached.

'As we go deep in the earth's crust the rocks generally assume schistose structure, we have reason to believe that the elastic constants of the constituent rocks increase in a certain particular direction, which evidently coincides with that of swiftest propagation of elastic disturbance. Pressed by the weight of the superincumbent crust these rocks will be of greater density, so that the increase of elastic constants is attended with corresponding increase of density. We cannot conceive that the elastic constant nor the density will continually increase as we approach the centre of the earth; they will both attain asymptotic values. The alternatives are, either the ratio of elastic constants to density goes on gradually increasing, or it first reaches a maximum and then goes on decreasing. The former supposition makes the velocity of elastic waves increase from the surface towards the centre of the earth, while the latter implies the existence of the *stratum of maximum velocity of propagation*. Such a stratum, if it exists, will lie pretty deep in the earth's crust and will be inaccessible to us, but the question will be settled by the seismologists.'

So far as I can discover, no real argument is advanced in favour of the hypothesis of the stratum of maximum velocity, which hypothesis is, however, taken for granted in the subsequent discussion.

In the above paragraph there are several points which might be criticized; such, for example, as the assumption that the schistose character of our superficial rocks will be characteristic of the deeper parts of the crust where presumably many earthquakes originate. As regards the main issue I am not aware of any reason which forbids us conceiving that both the density and the elastic moduli increase as we approach the centre of the earth. The probability is that both do increase, but that their ratio tends to an asymptotic value becoming practically constant long before

the centre is reached. This most probable case is excluded from the so-called 'alternatives', the second of which seems to me to be the least probable of all.

In the preceding paragraphs the facts and theories of the new seismology have been presented mainly in their historic setting. It remains now to take as complete a view as possible of the whole problem.

In the first place the evidence seems to me to be overwhelming in favour of the view that the preliminary tremors are not transmitted over the surface or even parallel to the surface, but pass by brachistochronic paths through the interior of the earth in accordance with the well-known laws of refraction of wave-motion.

The large waves seem to be true surface waves in the sense that they are transmitted through the outer crust, so that the apparent speed of propagation along the surface is a true speed.

If, however, the elastic waves which constitute the preliminary tremors are transmitted by brachistochronic paths through the body of the earth, emerging at various angles of incidence at the surface, then it is obvious from the first principles of wave-motion that the apparent speed of transit along the surface is not a real speed of propagation. It is the speed of the *trace* of the wave as it runs along the surface. Imagine the wave-front to be impinging internally on the earth's surface at an angle, say, of 30° with a speed of six kilometres per second. Then a simple geometrical construction shows that as the wave advances through ten kilometres, its trace on the surface advances through twenty. The angle which the wave-front makes with the surface is the complement of the angle between the surface and the direction of propagation; and the ratio of the real speed of propagation to the apparent speed of transit of the trace along the surface is the cosine of this so-called angle of emergence. This relation lies at the root of the investigations into the reflexion and refraction of wave-motion at the boundary of two media as treated in the last chapter. It is used by Green in his paper on the reflexion and refraction of light, and is no doubt as old as

Fresnel.[1] Nevertheless we find Dr. Benndorf in a paper on the transmission of earthquake waves (1906) demonstrating the theorem analytically and enunciating it as if it were altogether new in its generality. In a footnote he credits von Kövesligethy with proving it for a particular case in 1905.

It is generally supposed that the first preliminary tremors correspond to the compressional waves (see chapters ix and x) transmitted through the body of the earth. On this hypothesis the vibrations are in the direction of propagation, and will tend to cause a surface movement in the direction of the angle of emergence. The horizontal motion which is commonly measured will be only one component of the complete movement, and will be derived from it by multiplying by the cosine of the said angle.

The first investigator who attempted to measure directly the angle of emergence of the preliminary tremors was Schlüter,[2] who deduced it from the measured vertical and horizontal displacements. The results are interesting, although we cannot ascribe to them any very great accuracy. For the sake of comparison with the theory to be given presently, they are tabulated below, the first row being the arcual distance from the epicentre and the second the measured angle of emergence.

Angles of Emergence (Schlüter)

| | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Arc in degrees . . | 9 | 10 | 11 | 13 | 34 | 36 | 38 | 40 | 43 | 51 | 63 |
| Emergence-angle . | 29 | 39 | 56 | 59 | 64 | 69 | 73 | 75 | 78 | 78 | 80 |

Basing on these results Benndorf[3] shows that the time graph can be approximately reproduced by application of the relation connecting the apparent surface speed and the real speed at the surface, when that real speed is assumed to be 5·5 kilometres per second.

The general fact established by Schlüter's results is that the angle of emergence increases with the arcual distance, a

[1] It is part and parcel of the elementary explanation of total reflexion according to the undulatory theory.

[2] 'Schwingungsart und Weg der Erdbebenwellen', *Beiträge zur Geophysik*, vol. v, 1903.

[3] 'Fortpflanzung der Erdbebenwellen', *Mitteilungen der Erdbeben-Kommission der kaiserl. Akad. der Wissensch. in Wien*, No. 31, 1906.

fact in complete accordance with the view that the speed of propagation of the compressional wave increases with the depth. For arcual distances approaching 180° the angle of emergence will be so great that the cosine will be small and the horizontal component of the displacement correspondingly small and difficult to measure. This at once explains the frequently uncertain character of the first appearance of the tremors at great distances, and shows how important it is that the vertical component also should be measured. From a careful study of the comparative behaviour of various types of instrument Milne concluded, in 1903, that the observed times, as given in the table on page 229, should be considerably diminished; and this view is supported by Benndorf in the paper already cited. From the evidence of the vertical motion Benndorf supplies corrections which agree fairly well with those given by Milne in 1903; and in the subsequent discussion we shall take these corrected values as the most satisfactory data at our disposal. They are given in the following table.

Corrected Times of Transit of Preliminary Tremors (Milne and Benndorf)

| Arc | First Phase | Second Phase |
|---|---|---|
| 30 | 5·2 min. | 11·9 min. |
| 60 | 9·8 ,, | 17·5 ,, |
| 90 | 13·1 ,, | 23·8 ,, |
| 120 | 15·3 ,, | 27·5 ,, |
| 150 | 17 ,, | 30·5 ,, |
| 180 | 18 ,, | 31·3 ,, |

Within the last ten years there have been many suggestions as to the paths pursued by seismic radiations through the earth. Some ten years earlier, however, in 1888, Schmidt drew forcible attention to the probability that the speed of propagation of earthquake disturbances in the neighbourhood of the epicentre increased with depth below the surface. This implied that the paths of the seismic waves would necessarily be curved concave upward. Consequently the emergence angle of the shock at any locality could not be applied in the simple way at first imagined to the determination of the position of the focus. When towards the end of

last century the transmission of seismic vibrations all over and through the earth came to be recognized, the problem of brachistochronic paths hitherto confined to optics had at once a geological and seismological bearing. As a piece of mathematics the problem may be thus stated :—Given the law of change of speed of propagation with depth, find the form of the rays.

In 1898, basing on Milne's earlier results, I drew a sketch of the probable forms of wave-fronts and paths of propagation of compressional vibrations, which was published in the British Association Report for 1899. These curves were drawn by a tentative process in accordance with an assumed law connecting speed with depth ; for the material then to hand seemed too meagre to form the basis for a more formal attack. About the same time Rudzki published a paper in which the brachistochronic equations were worked out for a special integrable case, which did not, however, correspond with actuality. Somewhat earlier R. von Kövesligethy[1] had worked out the equations of radiation on the assumption that the speed of propagation diminished with the increase of depth. He was thus led to elliptic forms for the rays. In his second paper he shows how this can be made to harmonize with observation. Von Kövesligethy's mathematical work is admirable as the solution of a definite problem in wave propagation ; but there is certainly no physical basis for the implicit assumption that the speed of propagation of the elastic waves decreases as the depth increases, simply because the density is known to increase.

Benndorf's paper already cited is undoubtedly the most important contribution to the subject made in recent years. Making certain plausible assumptions as to the limiting values of the quantities involved, he deduces synthetically a law of velocity change with depth, which satisfies the values of the angle of emergence as determined experimentally by Schlüter. It is clear, of course, that an accurate knowledge of the angles of emergence can give no exact in-

[1] 'Neue Geometrische Theorie seismischer Erscheinungen', *Math. u. naturwissensch. Berichte aus Ungarn*, 1897. (The paper was presented in 1895. See also a later paper in the same publication for 1905.

formation regarding the speed of propagation in the deeper parts of the earth; and Benndorf's solution is only one of many. The particular law of velocity change with depth to which he is led is indicated in the following table, in which $x$ represents the distance from the centre in fractions of the earth's radius and $v$ the speed in kilometres per second.

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| $x$ | 0·0 | 0·2 | 0·4 | 0·6 | 0·8 | 0·9 | 0·95 | 0·975 | 1 |
| $v$ | 15·7 | 15·7 | 15·7 | 14·5 | 11·3 | 11·0 | 10·3 | 8·8 | 5·5 |

As the distance from the centre increases the speed of propagation remains fairly constant up to nearly half the radius, then it diminishes with increase of distance, at first slowly, then more rapidly, then more slowly again, till about the distance $x = 0{\cdot}95$. From this distance to the surface the speed diminishes with great rapidity from 10·3 to 5·5 kilometres per second.

I propose now to consider the problem in a direct manner, with the view of discovering to what extent a fairly simple assumption of the law connecting depth and speed of propagation can be made to agree with the facts of observation. I shall state the assumptions and give the conclusions, referring to a paper published in the *Proceedings of the Royal Society of Edinburgh* (1908) for the mathematical investigation.

The problem can be treated with great simplicity as an example of Hamilton's general method of system of rays. Two cases are considered. In the first case the speed at depth $x$ is represented by the formula $v^2 = V^2 - \mu^2 x^2$ and the values $V$ and $\mu$ are found so as to satisfy the surface value for $v$ and the time of transmission across a diameter. After some trials I chose the expression

$$v = 13{\cdot}6\sqrt{1{\cdot}2 - x^2}$$

as satisfying fairly well the two conditions named. Starting from this expression we can work out with ease the forms of the wave-fronts and of the rays, the times taken to pass along these rays, the angles of emergence, and the final distribution of energy over the surface. In the following table the more important quantities are given along with the minimum radius vector ($x$) of the ray corresponding to the arc $A$, with reference to which the other quantities are tabulated.

Case I. $v = 13{\cdot}6\sqrt{1{\cdot}2 - x^2}$ throughout the globe.

| Arc A° | Transit Time T | Minimum radius x | Emergence angle e° | Energy distribution over surface defined by arc |
|---|---|---|---|---|
| 5 | 1·6 | 0·998 | 10·3 | 3·26 |
| 10·1 | 2·9 | 0·98 | 26·6 | 20·04 |
| 15·6 | 4·3 | 0·958 | 36·4 | 35·26 |
| 24·9 | 6·5 | 0·913 | 47·9 | 54·99 |
| 37·7 | 8·8 | 0·841 | 57·6 | 71·24 |
| 49·6 | 10·6 | 0·775 | 63·4 | 80·03 |
| 65 | 12·4 | 0·684 | 69·0 | 87·22 |
| 98·7 | 14·9 | 0·492 | 73·4 | 95·02 |
| 144·2 | 17·5 | 0·214 | 84·9 | 99·21 |
| 161·7 | 17·8 | 0·109 | 87·4 | 99·82 |
| 170·9 | 17·9 | 0·055 | 88·7 | 99·97 |
| 178·2 | 18 | 0·011 | 89·8 | 99·99 |
| 180 | 18 | 0 | 90 | 100 |

Comparing the times of transit with the corrected times as given by Milne and Benndorf, we see that for arcs smaller than 100° the above table gives distinctly too high values. For arcs greater than 100° the values are in good agreement. This shows that the speed of propagation must alter more rapidly in the outer layers than is indicated in the formula assumed for $v$.

This suggests the second case, in which the formula

$$v^2 = V^2 - \mu^2 x^2$$

is taken as holding from the surface to a depth of one-tenth of the radius, the speed remaining constant at all greater depths. After a few trials I chose the expression

$$v = 24{\cdot}45\sqrt{1{\cdot}06 - x^2}$$

as holding true from $x = 0{\cdot}9$ to $x = 1$. At the surface where $x = 1$ the speed is 6 kilometres per second; and at all distances from the centre up to $x = 0{\cdot}9$ the speed is 12·23 kilometres per second.

The ray will be wholly curved when it does not penetrate deeper than $x = 0{\cdot}9$; but when it penetrates deeper than this distance the middle portion will be straight. In the following table, when the arc and time are expressed by the sum of two numbers, the first refers to the curved beginning and end of the path, the second to the straight and middle portion.

Case II. $v = 24{\cdot}45\sqrt{1{\cdot}06 - x^2}$ through the outer shell of one-tenth of the radius.

| Arc A° | Transit Time T min. | Minimum radius x | Emergence angle e° | Energy distribution over surface defined by arc |
|---|---|---|---|---|
| 1·7 | 0·4 | 0·999 | 11·5 | 8·03 |
| 6 | 2·0 | 0·977 | 42·4 | 45·96 |
| 17·9 | 3·9 | 0·922 | 60·7 | 76·1 |
| 21·6 | 4·5 | 0·9 | 63·8 | 80·55 |
| 14·2+ 38·5 | 3·5+ 5·1 | 0·85 | 65·4 | 82·65 |
| 10·5+ 67·1 | 3 + 8·6 | 0·75 | 68·4 | 86·49 |
| 6·7+111·4 | 2·5+12·9 | 0·50 | 75·8 | 94 |
| 2·6+146·2 | 2·4+15·3 | 0·25 | 83·0 | 98·5 |
| 180 | 2·3+15·6 | 0 | 90 | 100 |

The comparison of these two cases, with the actual case as represented by Milne's and Benndorf's corrections, is given in the following concise table derived graphically from the foregoing results.

| Arc | Times of First Phase | | |
|---|---|---|---|
| | Case I | Case II | Milne and Benndorf |
| 30 | 7·5 | 5·7 | 5·2 |
| 60 | 11·9 | 9·5 | 9·8 |
| 90 | 14·5 | 13 | 13·1 |
| 120 | 16·4 | 15·6 | 15·3 |
| 150 | 17·6 | 17·6 | 17 |
| 180 | 18 | 17·9 | 18 |

The values for Case II are in very good agreement with those derived from observation. That is to say, the observed facts of seismic radiation can be co-ordinated on the assumption that throughout all but a comparatively thin crust of the earth the elastic waves of highest speed are transmitted with a speed of fully 12 kilometres per second, and that within this crust of thickness equal to one-tenth the radius the speed decreases from the value 12·23 at the inner surface to the value 6 at the outer surface.

If this wave of highest speed be a compressible wave with longitudinal vibrations, the cosine of the angle of emergence of the ray will give the ratio of the magnitude of the horizontal motion to the whole amplitude. Now most of the observations have been obtained with instruments recording

horizontal motion only, and comparatively few with instruments recording vertical motion. But at great arcual distances from the epicentre the angle of emergence increases towards 90°, with corresponding diminution in the value of the cosine, and the horizontal component of the displacement will be very small, ultimately vanishing at the antipodal point. At such great distances there will consequently be a tendency for the preliminary tremors (assumed to be mainly compressional) to be retarded in their arrival. As a matter of fact it is, as we have seen, extremely difficult at times to determine the exact moment at which the tremors begin on records which have been obtained at stations further distant than 100° from the epicentre.

According to the theory here developed, we may obtain the times for the second phase of the preliminary tremors by increasing the times for the first phase in the ratio of 31·3 to 18, assuming for the purpose Benndorf's corrected values. The results are as follows.

Times of Transit of Second Preliminary Tremors

| Arc | Case I $v=8{\cdot}8\sqrt{1{\cdot}2-x^2}$ | Case II $v=14{\cdot}05\sqrt{1{\cdot}06-x^2}$ | Benndorf |
|---|---|---|---|
| 30° | 13 | 9·9 | 11·9 |
| 60° | 20·7 | 16·5 | 17·5 |
| 90° | 25·2 | 22·6 | 23·8 |
| 120° | 28·5 | 27·1 | 27·5 |
| 150° | 30·6 | 30·6 | 30·5 |
| 180° | 31·3 | 31·1 | 31·3 |

This gives 3·45 kilometres per second for the surface speed, and 7·03 for the greatest speed in the interior.

The agreement here is not quite so good as in the case of the first phase. It could be improved by slightly increasing the depth of the shell through which there is variation of speed of propagation. Viewed as a problem in elasticity this would mean that the modulus on which the speed of the second phase depended varied with depth according to a law not quite the same as that which held for the modulus determining the propagation of the first phase. The numbers for Case I are in somewhat closer agreement with observation than the like numbers for the first phase.

If the second phase be due to the arrival of the first tremors of the distortional wave, with displacements perpendicular to the direction of propagation, then the horizontal component of the displacement will be equal to the maximum displacement multiplied by the sine of the angle of emergence. Consequently, at great arcual distances, the second phase should tend to become proportionately in greater evidence than the first phase. There are, indeed, cases in which the second phase has been mistaken for the first, the latter having had too small a horizontal component to produce a record.

It seems to me that the theory here sketched is amply sufficient to co-ordinate all the known phenomena. The first phase of the preliminary tremors is thus identified with the compressional waves passing through the body of the earth. No doubt, especially in the more heterogeneous crust, surfaces of discontinuity will in the manner explained in chapter x start waves of distortional type along with the incident waves of compressional type. But across the practically homogeneous nucleus the compressional waves will run ahead of their associates of other type, so that what emerges at the surface, although modified in detail, must be referable to these compressional waves. Similarly when, somewhat later, the distortional waves flow in in quantity, there will be mingled with them waves of the compressional type. Nevertheless the second phase will be mainly composed of disturbances which have passed through the homogeneous nucleus as distortional waves, but have emerged modified in detail by refraction across discontinuous surfaces.

The distortional waves need not necessarily be more energetic than the compressional; but their generally greater amplitude as measured on instruments recording horizontal motion is at once explained in terms of the angle of emergence. From the data on pages 247–8 we find that for arcual distances greater than 60° the sine of the angle of emergence is greater than the cosine in ratios exceeding the value 2, rapidly increasing for greater arcual distances.

The longer periods which observation proves to be associated with the second phase do not seem to find an immediate explanation along the lines of this theory. But may these

not be explained as due to the intermingling of the quicker distortional vibrations with the less rapid compressional vibrations, which, because of their longer period, have travelled slower than the compressional waves of quicker vibration? True, the mathematical theory of elasticity does not recognize any relation between speed of propagation and length of wave; but this theory is only a first approximation to reality, and proves nothing either one way or the other as to what may occur in seismic vibrations. The fact that the first phase when well developed always begins with comparatively rapid oscillations seems indeed to establish the truth that shorter waves do travel faster than longer waves. If we take four seconds to be the shortest period we find that the disturbance travelling with a speed of 12·23 kilometres per second will have a wave-length of nearly 49 kilometres.

It may be of some interest to compare the elastic constants of the material of the nucleus of the earth on the assumption that we are dealing with compressional and distortional waves. The ratio of the speeds of the two types is 31·3 to 16 or 1·74 to unity. The ratio of the wave-moduli will be as the square of this or almost exactly 3 to 1. Hence in the notation of chapters ix and x we have

$$k + 4n/3 = 3n$$

where $k$ is the incompressibility and $n$ the rigidity. This gives

$$3k = 5n$$

a noteworthy result, showing that the inner parts of the earth almost accurately fulfil the conditions of isotropy possessed by the ideal elastic solid of Navier and Poisson. This conclusion seems to me to be an additional argument in favour of the view now being presented.

Here in the heart of the earth is a material at a high temperature and under great pressure, brought into a physical state suggesting homogeneity, though not necessarily implying it. As shown long ago by Tait, this globe is held together mainly by gravitational attraction. The cohesion between the molecules is, however, the force which is involved in the propagation of the elastic disturbances which radiate from a seismic centre. The view of the French elasticians was that true homogeneity required

a definite relation between incompressibility and rigidity. This definite relation is not realized in the case of materials tested by ordinary combinations of stress and strain. This fact, however, was not admitted by de St. Venant as disproving the uniconstant theory developed by Navier and Poisson; for as soon as an aeolotropic stress is applied to our rods and wires, the material ceases to be truly isotropic.

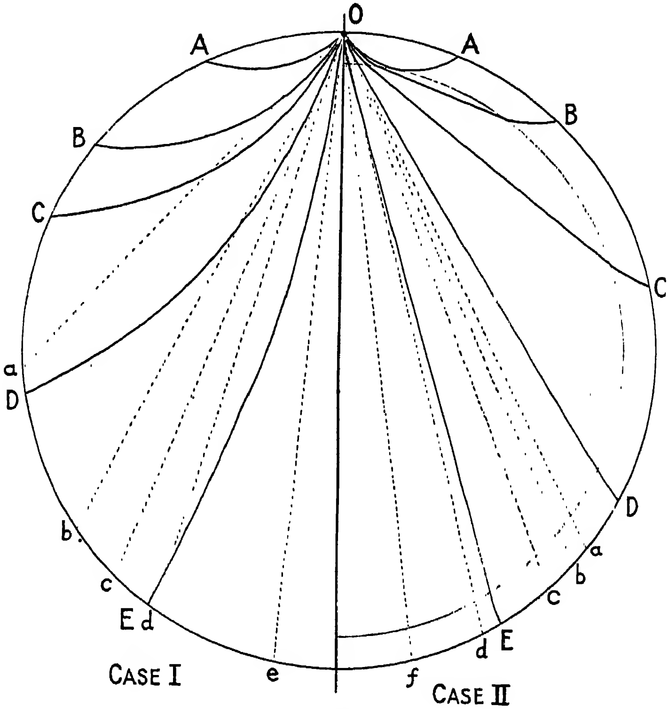



FIG. 43.

It would appear from the calculation just made that the interior of the earth is in a condition which at each point might be described as isotropic; and the relation required by the uniconstant theory is accurately satisfied by the constants calculated from the propagation of the two phases of the preliminary tremors when these are assumed to be respectively the compressional and distortional vibrations.

We shall now consider the significance of the last column

of figures in the tables on pages 247 and 248, the columns headed Energy Distribution. The meaning of the figures is best explained by consideration of particular cases in connexion with the foregoing diagram.

The diagram represents a section of the globe, and some of the particulars corresponding to each of the cases are figured on the one half. The full lines show the paths of the seismic disturbances as they radiate out from the origin O. Each ray corresponds to one of the particular set of values tabulated on pages 247, 248. The following short table gives the value of the arc corresponding to each ray, the rays being represented by the terminal letter on the diagram:—

| Ray | A | B | C | D | E |
|---|---|---|---|---|---|
| Case I . . . | 24·9 | 49·6 | 65·0 | 98·7 | 144·2 |
| Case II . . | 21·6 | 52·7 | 77·6 | 118·1 | 148·8 |

To each full line OA, OB, OC, &c., there corresponds a dotted line Oa, Ob, Oc, &c., which starts tangential to the curved ray and is therefore the direction in which the disturbance begins to radiate outwards from the origin. Considerations of symmetry show at once that the angle which each dotted line makes with the surface at the origin is the same as the angle with which the ray emerges at the other end. In other words, this angle is equal to the emergence angle as tabulated above.

In the diagram the left-hand semicircle shows the rays for the first case, in which the variation of speed is assumed to take place throughout the whole globe; and the right-hand diagram the second case, in which the variation takes place only through the upper layer of thickness equal to one-tenth of the radius. In the latter case the first ray OA lies wholly within the layer of varying speed of propagation, and is curved throughout its whole length. All the other rays represented pass partly through the interior of constant speed of propagation and are straight throughout a part of their course. Thus the rays OD, OE to distant points are very approximately coincident with chords, but for shorter rays such as OC and OB the chordal coincidence is not so close. We shall discuss this case in some detail.

The dotted line Oa in Case II gives the direction in which a ray would have passed if the speed of propagation had been absolutely constant throughout the whole globe. This condition would have given rise to what we may call the spherical distribution of energy over the surface of the globe, half the energy being in fact distributed over the hemisphere of which the origin is the pole. But in the case represented in the right-hand semicircle the ray starting originally along the dotted line Oa becomes bent round by refraction so as to assume the position OA. The energy, of course, passes with it. Hence the energy which in the simple case of spherical distribution would have been distributed over the part of the surface defined by the arc Oa with O as pole is concentrated within the much smaller part of the surface defined by the arc OA. We are to imagine Oa to be one of a cone of rays which divides the spherical surface into two parts, defined respectively by the arcs into which Oa divides the semicircle. The semivertical angle of this cone is equal to $90° - e$, where $e$ is the corresponding angle of emergence belonging to the ray OA; and the area on the spherical surface defined by the arc Oa is proportional to

$$1 + \cos (180° - 2e) = 1 - \cos 2e.$$

This number, divided by 2, the value when $e = 90°$, represents the fraction of the energy which is finally distributed over the surface defined by the arc OA. Thus in the particular case which has been the subject of discussion, 80·1 per cent. of the whole energy is found distributed over the comparatively small fraction of the surface whose boundary lies 21°·6 from the epicentre. In spherical distribution only 3·5 per cent. of the whole energy would have appeared over this surface. Glancing back to the table for Case II, we see that 50 per cent. of the whole energy is distributed over the small surface whose boundary lies about 7° from the epicentre, and that 75 per cent. is distributed over the surface which does not extend to 18° from the epicentre. In spherical distribution these surfaces would have received respectively only $\frac{1}{3}$ and $2\frac{1}{2}$ per cent. of the whole energy.

It is interesting to compare the two cases figured side by side on the diagram, and to notice how much more con-

centrated the energy is in the neighbourhood of the epicentre in Case II, which is characterized by a rapid variation of speed of propagation within the upper layers of the earth.

In these calculations I have, for simplicity, assumed the origin to be at the surface. This is never quite the case in large, world-shaking earthquakes. These originate at depths which may vary from 10 to 50 miles. Nevertheless, because of the curving of the seismic rays the energy will be distributed unequably in a manner similar to what is here indicated. The deeper the origin the less unequable will the final distribution be; but so long as the origin lies within the layer of changing velocity, there must be the curving round of the seismic rays, carrying their energy with them.

Let there be two earthquakes of equal intensity but with their origins at different depths. The one with the shallow focus will have its energy strongly concentrated towards the surface regions immediately in the vicinity of the epicentre; while the energy associated with the deeper focus will be less unequably distributed over the whole surface. The latter will be registered all the world over as a world-shaking earthquake, while the former may appear much more limited in its sphere of action, simply because of the small intensity of the tremors which pass to distant regions.

It would be possible, though somewhat laborious, to work out the surface distribution of energy for several depths of origin along the lines indicated above. If this were done, and if instruments could be constructed to give an accurate measure of the energy associated with seismic movements at the surface, we should be in possession of a method for determining the depths of origins—a problem which has hitherto baffled all endeavours.

In the case of earthquakes, various complications of wave-motion will come in to alter the character of the vibrations. For example, if, as is highly probable, the speed of propagation depends on the wave-length, there will be interference of wave-motions of nearly equal wave-length to produce the phenomenon of group-velocities. The speed with which the group advances will be greater or less than the speed of

propagation of the individual waves, according as this latter speed diminishes or increases with increase of wave-length. In all probability the speed of the purely elastic waves which run ahead of the earthquake disturbance is independent of the wave-length; but the evidence of the records seems to suggest that what we have called the quasi-elastic waves travel more slowly as the wave-lengths increase. Hence in such cases wave groups will advance through the waves at a somewhat greater speed. The existence of wave groups will have its own effect upon the character of the records producing appearances analogous to those of resonance. Also it is conceivable that the long wave-lengths present in the record may really be the intervals between groups of waves and not a true original wave-length in the ground.

Another kind of complication may arise from effects of dispersion. Nagaoka has considered this question in an interesting manner, and has found evidence in the seismograms of a phenomenon analogous to what is known as anomalous dispersion in optics.

It is sufficient to draw attention to some of the complicated accompaniments of waves in heterogeneous media in order to guard ourselves against basing conclusions upon a too simple and therefore incomplete theory of wave-motion.

One great fact established by these seismological observations is that vibrational disturbances can be propagated throughout the whole body of the earth. They must be regarded as elastic; and the fact of their propagation proves that in the deeper parts of the earth viscosity is of small account. Another fact of importance is the recognition of three distinct types of vibratory motion, the two kinds of preliminary tremors and the large waves.

I have several times expressed the view that the large waves are transmitted by a succession of reflexions within the crust from the upper surface which is backed by air or water. The result obtained both by Omori and Marvin (p. 239) as to the preponderance of transverse vibrations in the early phases of this portion of the earthquake record is of no little interest when looked at in the light of the theory of elastic waves as developed in chapter x. I refer

especially to the conclusions arrived at in connexion with reflexion and refraction of elastic waves at a boundary of rock and water or rock and air (pp. 175 to 181). In both these cases, for high incidences in the neighbourhood of 80°, the energy incident in the condensational type of wave is very largely transformed by reflexion into the distortional or transverse type, whereas the incident distortional waves are themselves largely reflected as distortional still. Thus whatever be the original type of the incident wave, successive reflexions at the high incidences necessarily imply an increasing proportion of transverse vibrations. Marvin's clear-cut result described above on page 239 is at once explained along the lines of the theory here advocated. The first phase of tremors passed directly from Kingston to Washington by concave paths through rock only, largely preserving their longitudinal character, and failing to produce a record on the EW. instrument. The second phase marked the influx of transverse vibrations superposed upon the longitudinal vibrations, and thus both instruments responded. The early portions of the large waves, which quite possibly began as longitudinal motion through the shallow parts of the crust, became by reflexion at the water-backed surface of the earth's crust so largely impregnated with transverse vibrations as to indicate their presence on the EW. instrument a few seconds earlier than on the NS. instrument. It is not to be expected, of course, that all the phenomena can be brought into line with the purely elastic theory. But there is a mental satisfaction in finding some point of resemblance between this theory and the results of observation.

The general problem of seismic vibrations set up in the earth is one of great complexity; for even the comparatively simple case of the vibrations of a homogeneous elastic sphere the size of our globe presents great difficulties. The fundamental modes of vibration of an elastic sphere were investigated in 1882 by Professor Horace Lamb in a paper which has become classical.[1] According to his results all modes of vibration can be grouped under two types, the one of which corresponds to the purely distortional type

[1] *Proceedings of London Mathematical Society*, vol. xiii.

referred to above. The other does not, however, correspond to the simple dilatational type, but involves rotational strain. The frequencies of the higher modes of vibration approximate in each case to the series of natural numbers 5, 6, 7, 8, &c.; but the series in the two types are not necessarily commensurable. Lamb shows that the slowest mode of vibration of a steel sphere the size of the earth would have a period of seventy-eight minutes. The higher modes will have higher frequencies, the period of the 21st mode being, for example, about one minute, of the 42nd half a minute, and so on. Now we have seen that periods may range from three or four seconds to nearly a minute. There is thus no difficulty in accounting for the presence of a number of different periods in earth tremors. The difficulty is rather to explain why the periods are so limited in number.

In the elastic sphere imagined by Lamb, a condensation produced at any locality will be resisted by the incompressibility of the material, and will start a series of condensational rarefactional waves. But if we suppose the sphere to be under its own gravitational action any alteration of density will, so far as gravitational effect is concerned, tend to be accentuated. Material will tend to move towards the denser region and away from the less dense. In other words, gravitation implies an instability which must be resisted by the elastic properties of the material. This consideration suggests the problem: what incompressibility is necessary in a sphere the size and mass of the earth in order that the gravitational instability may be checked? The question has been discussed by J. H. Jeans, Lord Rayleigh, and very recently by A. E. H. Love, in his paper on the Gravitational Stability of the Earth.[1] Having regard to the observed speeds of propagation of seismic radiations, Love finds that the moduli of elasticity are sufficiently great to render a spherically symmetrical configuration of the mass and size of the earth completely stable. An interesting feature of this paper is the mathematical representation of the land and ocean contour of the earth's surface.

[1] *Philosophical Transactions*, 1907.

# CHAPTER XIII

## MISCELLANEOUS RELATIONS

Miscellaneous Relations. Building Construction. Vibrations of Locomotives. Vibrations of Bridges. Suboceanic Changes. Destruction of Telegraph Cables. Milne's Comparison of Earthquake Frequency and Movements of Earth's Pole: Newcomb, Hough. Albrecht's Curve. Discussion of the Relation. Terrestrial Magnetism and Seismicity. Earthquakes and Volcanoes. See's Theory of Steam Explosions. Murray's Views of Elevation of Continents. Internal condition of Earth: Fisher, Chamberlin. Evolution of the Earth. Jeans and Sollas.

Our discussion of the physics of earthquake phenomena would not be complete without some reference to side issues both of a practical and theoretic interest.

One of the most important of these is the question of building construction in seismically active regions. In Italy and Japan the subject has been carefully studied for years; and the communities of San Francisco, Valparaiso, and Kingston (Jamaica) have, because of recent disasters, become alive to the necessity of a close consideration of the nature of the soil on which buildings are to be set up as well as the mode of construction of the buildings themselves.

A quotation from Professor A. C. Lawson's preliminary report on the San Francisco shock of April 18, 1906, will indicate the importance of this kind of investigation. It fully bears out what has been established by seismologists working both in Italy and Japan.

'In the city of San Francisco we may recognize for preliminary purposes four types of ground: (1) The rocky hill slopes; (2) the valleys between the spurs of the hills which have been filled in slowly by natural processes; (3) the sand dunes; (4) the artificially filled land on the fringe of the city. Throughout the city we have a graded scale of intensity of destructive effects which correspond closely to this classification of the ground. The most violent destruction of buildings, as every one knows, was on the made ground. This ground seems to have behaved

during the earthquake very much in the same way as jelly in a bowl, or as a semi-liquid material in a tank. The earth-waves which pass through the highly elastic rocks swiftly with a small amplitude seem in this material to have been transformed into slow undulations of great amplitude which were excessively destructive. The filled in material and the swampy foundation upon which it rests behaved, in other words, as a mass superimposed upon the earth's surface, rather than as a part of the elastic crust itself. In a less degree the naturally filled valleys between the hill spurs were susceptible to this kind of movement, and the destruction of buildings was correspondingly less, but still severe, depending very largely on the character of the buildings, the integrity of their construction, &c. In portions of these valleys, however, the original surface of the ground has been modified by grading and filling, and on the filled areas the destruction was more thorough than elsewhere in the same valley tracts. On the rocky slopes and ridge tops, where, for the most part, the vibration communicated to buildings was that of the elastic underlying rocks, the destruction was at a minimum. On some of the hills chimneys fell very generally and walls were cracked; on others even the chimneys withstood the shock.

'While this correlation of intensity of destructive effect appears to hold as a generalization there are well-known exceptions which find their explanation in the strength of the structures. Modern class A steel structures with deep foundations appear to have been relatively passive, while the made ground in their immediate vicinity was profoundly disturbed. Thoroughly bonded and well cemented brick structures, on similarly deep and solid foundations, seem to have been equally competent to withstand the shock, except for occasional pier-like walls not well tied to the rest of the building. The weak points in wooden frame structures were in general the faulty underpinning and lack of bracing, and chimneys entirely unadapted to resist such shocks. With these faults corrected, frame buildings of honest construction would suffer little damage beyond cracking of plaster in such a shock as that of April 18, save on the made ground, where deep foundations and large mass appear to be essential for the necessary degree of passivity.'

In short, a good rock foundation and sufficient bracing to impart practical rigidity to the building are among the most obvious requirements in earthquake countries. Heavy

ornamental copings and tall chimneys are to be shunned, since they tend to be set into swinging motions which reinforced by resonance may lead to collapse.

If a rock foundation cannot easily be got, some amount of protection is given by digging trenches all round or on the side from which earthquake shocks usually come.

These are precautions which ought to be taken in every land subject to frequent earthquakes. With moderate shocks little damage will result. And even with large destructive quakes, which are fortunately only occasional visitants, the destruction will be minimized by attention to these important points.

An interesting application of seismometry was made by Milne to the investigation of the vibrations of locomotive engines travelling along a railway track. By means of a not very delicate form of seismometer placed on the engine he obtained tracings which varied in magnitude according to the position of the track, indicating at once where the less perfect parts were. The work has been continued by Omori with improved apparatus; and valuable results have been obtained bearing upon various questions, such as the relation between the vibration of carriages and the speed of propulsion, the effect of curvature, the comparison of the vibrations of the carriage as it passes across a bridge or travels on the ground, &c. There is necessarily a considerable amount of friction in such vibration recorders, so that the records may be assumed to be fair reproductions of the motion of the carriage. In certain cases, however, there is evidence of resonance effects, which will begin to be apparent when the forced vibration due to the motion of the carriage has a period coming within, say, 30 per cent. of the period of the so-called 'steady points'.

We have seen that the majority of earthquakes originate beneath the ocean. It is therefore obvious that earthquakes may have destructive effects on telegraph cables laid along the bottom of the sea. In the Aegean Sea, in the coast off South America and in the West Indies, there are frequent cases of the snapping of telegraph cables, and some of these are no doubt to be attributed to landslips or

earthquakes in the bed of the ocean altering the configuration and straining the cable till it breaks. In the seismological report for 1897 laid before the British Association Milne has attempted an analysis of cable dislocations in relation to seismic disturbances. It appears that there is on the average one dislocation per year for every 434 miles of cable laid. If the majority of these cable dislocations be due to landslips and earthquakes, Milne reckons that about 300 of such suboceanic disturbances must occur along our coast lines in the course of the year.

Some attention has been paid to a possible connexion between earthquakes and magnetic changes. The evidence has been carefully collected by Milne in several of the British Association Reports; but the only certain conclusion that can be drawn is that when the seismic disturbance is large enough the magnetographs act to some extent like seismometers. In other words, the effects were dynamical, not magnetic. In the Indian earthquake of April 4, 1905, quite distinct disturbances were observed on the magnetographs at Bombay; but an inspection of these quite bears out the opinion of the director, Mr. Moos, that they are mechanical effects.

Another curious inquiry taken up by Milne is into a possible connexion between the secular movements of the earth's axis and the occurrence of large earthquakes. The movements of the earth's axis are indicated by the slight changes of latitude which are observed to take place at astronomical observatories. The investigation is a difficult one, dealing as it does with small quantities which must be 'corrected' for known causes of change; and the residuals are in the opinion of some too small to base any argument upon. Taking Albrecht's representation of the irregular spiral-like motion of the projection of the earth's axis on the celestial sphere, Milne showed that there was a preponderance of large earthquakes at times at which the direction of motion of the projection of the axis changed rapidly. In other words, there was a hint that a more than average acceleration of the motion of the earth's pole seemed to be associated with a more than average seismic

activity. In the British Association Report for 1906, Milne has put the argument in a somewhat better form. I reproduce from the same report Albrecht's figure of the path of the pole from 1899·9 to 1905·0, showing a fairly regular spiral movement. The large numbers on the figure are Milne's additions, giving the number of world-shaking earthquakes which occurred during the particular interval of time along which the number is entered.

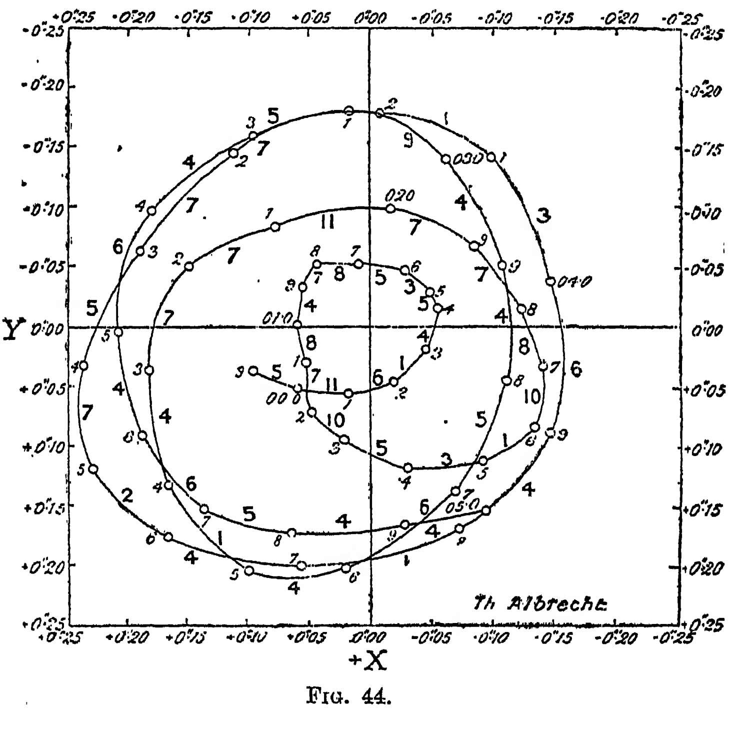



FIG. 44.

A similar diagram for the period 1892 to 1899 was published in the British Association Report for 1903. The motion of the pole during the earlier interval of eight years was much more irregular than its motion during the second interval of five years. Treating these two intervals separately, Milne tabulates the number of large earthquakes which occurred for definite integral curvatures of the path for each tenth part of the year. The integral curvature

is the total change in direction as we pass from the beginning to the end of the part of the path considered. This Milne calls the deflection. He then takes the deflections in successive groups of 5°, and counts the number of deflections which occurred in each group and the corresponding number of world-shaking earthquakes. The details are given in the two following tables, one table being for the earlier eight years and the other for the later five years.

| Deflections | 1892–1899 | | | 1900–1905 | | | 1892–1905 | |
|---|---|---|---|---|---|---|---|---|
| | No. of Defns. | No. of Eqks. | Aver. No. | No. of Defns. | No. of Eqks. | Aver. No. | No. of Eqks. | Aver. No. |
| 0° to 5° | 1 | 18 | 18 | — | — | — | 18 | 18 |
| 5° to 10° | 7 | 153 | 22 | 1 | 8 | 8 | 161 | 20·1 |
| 10° to 15° | 8 | 32 | 4 | 2 | 33 | 16 | 65 | 6·5 |
| 15° to 20° | 4 | 13 | 3 | 3 | 39 | 13 | 52 | 7·4 |
| 20° to 25° | 13 | 128 | 9 | 2 | 18 | 9 | 146 | 9·7 |
| 25° to 30° | 10 | 185 | 19 | 13 | 134 | 10 | 319 | 13·9 |
| 30° to 35° | 9 | 115 | 13 | 8 | 89 | 11 | 204 | 12 |
| 35° to 40° | 11 | 137 | 13 | 5 | 62 | 12 | 199 | 12·5 |
| 40° to 45° | 5 | 67 | 13 | 5 | 43 | 8 | 110 | 11 |
| 45° to 50° | 6 | 115 | 19 | 2 | 21 | 10 | 136 | 17 |
| 50° to 55° | 6 | 120 | 20 | — | — | — | 120 | 20 |
| 55° to 60° | 2 | 42 | 21 | — | — | — | 42 | 21 |
| 60° to 75° | 5 | 122 | 24 | — | — | — | 122 | 24 |

The greater irregularity of the motion of the earth's pole during the earlier period is shown by the greater variety of deflections of the path per tenth year. Multiplying the mean of each range of 5° by the number of times that particular range of deflections occurred and dividing by the total number of occurrences we find the mean deflection during the period. It comes out 30°·6 in the earlier period and 30°·2 in the later, a very satisfactory agreement. Milne's idea is to connect the occurrence of large earthquakes with the curvature; and the above table seems at first glance to bear out the conclusion that there were more earthquakes when the pole was subject to greater normal acceleration. But if there be any connexion between large earthquakes and the movements of the pole, ought we not rather to consider the deviations from the mean value of the deflections themselves? This mean curvature per tenth year we may consider to be due to some steadily

acting dynamical cause, such as a slight departure from coincidence of the axis of rotation with the principal axis of inertia. From this point of view we should regard small deflections of 5° or 10° as being abnormal equally with large deflections of 60° or 70°. Hence the earthquake frequencies should be compared with the deviations of the deflections from the mean, which for the whole set of observations is almost exactly 30°·5. Taking the differences between this mean and the average deflections in each range we obtain what may be called the 'deviations' from mean curvature. These form the first column in the following table, the second column giving the number of times this particular deviation occurred, and the third column containing the average number of earthquakes corresponding to the deviation.

| Deviation from Mean | Number of Occurrences | Average Number of Earthquakes |
|---|---|---|
| −28 | 1 | 18 |
| −23 | 8 | 20·1 |
| −18 | 10 | 6·5 |
| −13 | 7 | 7·4 |
| − 8 | 15 | 9·7 |
| − 3 | 23 | 13·9 |
| + 2 | 17 | 12 |
| + 7 | 16 | 12·5 |
| +12 | 10 | 11 |
| +17 | 8 | 17 |
| +22 | 6 | 20 |
| +27 | 2 | 21 |
| +37·3 | 5 | 24 |

The second column of figures is given by way of contrast. It is obvious that they follow roughly the well-known law of grouping about a mean, the maximum being in the neighbourhood of zero deviation. It is quite otherwise with the earthquake numbers. If there were no connexion of the kind looked for the average number of earthquakes would be the same for all deviations. But there seems to be a tendency to greater values for greater deviations. Thus for deviations up to +12 and −13, the averages total 66·5 with a mean of 11·1. For deviations greater than these limits the averages total 126·6 with a mean of

18·1. This conclusion lends a certain amount of support to Milne's view that there is some connexion between the occurrence of large world-shaking earthquakes and the movements of the earth's pole.

The mean curvature of the path of the pole is 30·5° per tenth year, or 305° per annum. Hence the pole will make a complete revolution in $365 \times 360/305$ days or 432 days. The value given by Chandler is 427 days. It is well known that a rigid body of the size, figure, and mass of the earth will have a small precessional motion of period 305 days if the axis of figure is not quite coincident with the axis of diurnal revolution. To explain the large discrepancy between the observed value 427 and the theoretical value 305, Newcomb invoked the influence of elasticity in modifying the period of precessional rotation. His original calculation was admittedly approximate; and Hough has worked out the problem[1] in a more rigorous manner. Taking account of the elasticity only he finds that the precessional period will have the value 427 days if the effective rigidity of the earth were a little greater than that of steel. Newcomb also pointed out that the mobility of the ocean would have the same effect of lengthening the precessional period. Further, if the effective rigidity of the earth were to diminish all over, the precessional period would be increased. It is not easy to see what would be the immediate effect of either a local diminution of rigidity, or a local yielding to stresses, such as takes place when an earthquake is originated. But it is at all events not unreasonable that some effect will be produced. This probably is the direction in which we must look for the connexion imagined by Milne.

It should be noted that Hough's calculation is based on the assumption that the material of the globe is incompressible. His conclusion is that, if $T$ is the precessional period for a perfectly rigid globe and $T'$ the precessional period for the same sized globe when its material yields to shearing stresses, then the ratio is given by

$$T'/T = 1 + 2ga/19u^2,$$

[1] 'Rotation of an Elastic Spheroid', *Philosophical Transactions*, vol. 187, A: 1896.

where $a$ is the earth's radius, $g$ the acceleration due to gravity, and $u^2$ the ratio of the rigidity to the density. Now observation gives $T'/T = 427/305$. Consequently we find $u = 400{,}000$ centimetres per second, or 4 kilometres per second. It may be assumed that the parts of the earth which have most effect in producing this change of period are those furthest away from the axis of rotation, that is, in the superficial equatorial regions. Hence we should regard this speed of 4 kilometres per second or 2·5 miles per second as the average speed of the transverse elastic wave through the superficial parts of the earth. This harmonizes with the values obtained in last chapter, p. 249. Also the highest values of the rigidity measured by Kusakabe are 52·2 and 49·0 ($10^{10}$ C.G.S. unit) in an Archaean Serpentine and a Palaeozoic Pyroxenite respectively. Their densities being 2·71 and 2·9, the corresponding speeds of propagation of the distortional wave are 4·4 and 4·33 kilometres per second. The agreement is certainly remarkable, although it must be remembered that Hough's calculation cannot apply without reservation to the earth, the material of which is to an appreciable extent compressible.

The observed speeds of transit of earthquake disturbances in the neighbourhood of the epicentre vary considerably, being generally less than 2 miles per second, but in the case of the Charleston earthquake reaching the high value of 3·25 miles per second (see above, p. 35). In 1885, when the Flood Rock was destroyed by an artificial explosion, General H. L. Abbot took the opportunity of measuring the rate of transmission of the tremors started by the explosion. The highest speed of transmission obtained was nearly 3·9 miles per second. It was believed, however, that higher speed would have been obtained with more delicate seismoscopes. Explosion experiments have been made by various investigators, notably Mallet, Milne, Fouqué, and Levy; but the speeds of transmission obtained in these experiments were much smaller than that found by Abbot. Probably the strength of the explosion is an important determining factor.

The possible relationship between earthquakes and volca-

noes has exercised the minds of naturalists all through the ages. There is the undoubted fact that volcanoes are frequently prominent features of countries which are subject to seismic disturbances. Many of the finest hills in Japan are either active or extinct volcanoes; and down the line of the Andes there is a succession of some of the grandest volcanic cones in the world. Similarly in the south of Italy volcanic eruptions and earthquake shakings are frequent visitants. But a careful consideration of the facts shows that in many cases the connexion between the two kinds of phenomena is not very close. The Japanese volcanoes form the backbone of the country, whereas the earthquakes mostly originate under the ocean to the east. The earthquakes which devastate the western coasts of South America have their origin under the neighbouring Pacific Ocean. The great earthquake of Lisbon was unassociated with any known volcanic eruption. The disasters which visit the northern parts of India from time to time occur in regions quite free from volcanoes.

When a large volcano like Aetna becomes more than ordinarily active, the surrounding district experiences tremors and shakings; and it is probable that a certain type of local earthquake may be comparable in its origin to the beginnings of a volcanic eruption. This has been generalized into the view that each earthquake is an incompleted effort to establish a volcano. Mallet evidently held such a theory; and the view is a very natural one. The volcano with its molten lava, its volumes of steam and sulphurous fumes, proves that the inner parts of the earth are at a high temperature; and where heat and vapour are we have all the conditions for violent ebullition and changing pressures. But we have no right to assume that these forceful changes of state are confined to the immediate neighbourhood of a volcanic vent. Is it not reasonable to suppose that such eruptive tendencies exist all through the crust of the earth, ready to declare themselves on a favourable opportunity? The favourable opportunity will come when barriers break down or weak parts yield under the moulding influence of aeolotropic stresses.

In a recent paper on the cause of earthquakes and kindred phenomena connected with the physics of the earth, Dr. T. J. J. See[1] has worked out in detail a theory which traces all volcanic and seismic action to the penetration of the ocean waters to the lower parts of the crust. The facts on which the theory is based are the geographical distribution of volcanoes and seismically sensitive regions along the margins of continents and islands, the great quantities of steam blown off during volcanic eruptions, and the association of lines of mountain ranges with coast lines demonstrating a community of origin. Under the action of the great pressures at the bottom of the deep sea the water penetrates the rock partly through fissures, partly by capillarity in the pores, partly by diffusion, and is believed to reach considerable depths where it is held absorbed by the material under very great pressures and high temperatures. Under favourable conditions this highly heated water becomes steam with explosive accompaniments, and the earthquake is produced. If the explosion is sufficiently violent it may break through the superincumbent material as a volcanic eruption. See regards volcanoes as a particular kind of mountain, all mountains being heaved up by the action of the same force, but becoming distinct volcanoes when the expanding steam and hot lavas are able to effect an exit. The theory is worked out with great ingenuity, and is probably the most powerful argument which has ever been advanced in favour of the view held by many down the ages that the expansive force of steam is the prime cause of volcanic eruptions and seismic disturbances. The secondary as well as the fundamental facts of mountain building and continent raising are skilfully marshalled in support of the theory. Thus the elevation of the land and the subsidence of the ocean bottom are explained as the result of the injection of lava beneath the land, the injection being produced by the explosion of steam underneath the ocean floor.

The percolation of sea water into the rocky sides of the ocean is a very probable phenomenon, and has often been invoked by geologists to explain the volumes of steam which

[1] *Proceedings of the American Philosophical Society*, vol. xlv, 1906.

accompany volcanic eruptions. Dr. See, however, goes further and assumes that the water leaks through the ocean floor, and descends to great depths in the heated semi-molten rock, being absorbed by the rock under the great pressures existing there. But are we warranted in assuming such a continuous leakage down to great depths? The hydrostatic pressure of, say, 1000 atmospheres at the deepest depths of ocean cannot be supposed to have any direct effect in forcing the water down; for the pressure gradient is upward and not downward. There are no air-tight cavities below the ocean floor into which the pressure could urge the water. The conditions in laboratory experiments, in which fluid is pushed through solids, are not satisfied in these great depths. Even if, in the initial stages, cracks and fissures aid the percolation of the water, the only certain process by which water may penetrate to deeper regions is the process of diffusion. It is not at all evident that water penetrating in this way in a state of combination with, or absorption by, solid matter will of itself have explosive tendencies. To account for the explosions we seem to require some other agencies for changing the pressure environment. This we may find in the aeolotropic stresses and strains which may well accompany the cooling of the globe.

Let us, however, postulate the periodic recurrence of explosions underneath the bottom of the ocean. The question to be answered is, how will these explosions act so as to push the steam-saturated lava underneath the continents and islands? Briefly put the argument seems to be this. Continents and islands are undoubtedly raised; sea-water filtering down to great depths may be supposed to come into a state which implies sooner or later an explosion; this explosion will displace material; and in order that elevation of land may occur this displacement must be mainly towards the land. When the land is piled up high enough the explosions begin to push the lavas in the opposite direction, causing the uprise of ocean ridges and islands.

Some of these views may be reasonable enough; but I should be inclined to modify Dr. See's theory to the extent of regarding the explosive evolution of steam as being itself

an effect of deeper seated causes relieving pressure in the more sensitive regions. The trigger, so to speak, is pulled, and the explosive force thereby engendered gives direction to the impulses through the surrounding rocks.

There can be little doubt, at any rate, that surface waters get absorbed by the hot lavas welling up from the levels to which, in virtue of their high temperature, we must regard them as belonging. Yet there is no reason against the view that some of the steam evolved at the volcanic vent may have been part of the original chemical constitution of the rock itself. In other words, all the steam need not necessarily have come originally from the sea.

We are not, indeed, limited to the assumption that steam or gas explosions alone are responsible for the continuous elevation of the continents. A simple, natural, and apparently effective mechanism was explained by Sir John Murray in 1899 in his address to the Geological Section of the British Association. His theory is based upon the nature of deep sea deposits, which contain much less silica than the rocks forming the dry land. On the other hand, lime, iron, and other bases largely predominate in the abyssal regions. This has been brought about, and is being now brought about, by a continual assortment among the mineral substances. The silicates of the crust are attacked by water and carbon dioxide, and robbed largely of their bases. These are carried out to the deeper parts of the ocean, forming beds of clay, of oxide of iron, and of manganese, while the silica, largely in the form of quartz sand, remains on or about the emerged land. Denudation produces not only an unloading over the land and a loading along the margins where the deposits accumulate, but also separates the lighter quartz from the heavier bases. The relief of pressure under the land and the increased pressure under the shore line where the deposits accumulate work together to produce a constant shift of light material towards the continental regions. Meanwhile, accompanying this upheaval there is the depression of the bed of the ocean, which is always becoming more basic and growing comparatively denser. In this changing configuration of the contour of our globe the sub-aerial

agencies of life, wind, and running water do their own share of work, supplementing and correcting the influences which have their origin in the deeper parts of a cooling and contracting world.

As regards the life-history and present state of the interior of the earth, especially in its relation to seismic phenomena, we can do little better than speculate. It will suffice to indicate a few of the more recent lines of research and speculation.

We may imagine the earth to have been, in its early stages, a somewhat diffused mass of meteors, meteoric dust, and gaseous emanations. This primeval earth gradually condensed, losing gravitational potential energy, which became transformed into heat. Density and temperature would thus tend to increase in the interior parts of the earth until a certain stage was reached, when loss by conduction, radiation, and convection prevented the further increase of temperature. The density would, however, still continue to increase, partly because of gravitation, but mainly because of contraction due to cooling. There would be a tendency to a distribution of density and temperature according to the distance from the centre, modified somewhat by the rotation of the earth on its axis. The isothermal surfaces would be on the whole coincident with the surfaces of equal density and pressure.

So far there is complete agreement among scientific men; but as regards the details of the process and the present distribution of temperature throughout the earth there is considerable difference of opinion. The facts on which we can build our theories and speculations are extremely meagre. We know practically nothing of the properties of matter under the enormous pressures which certainly exist, and the high temperatures which presumably exist in the nucleus of the earth. A close examination of any definite theory of the internal condition of our globe shows that it rests on assumptions which are to a large extent chosen because they are amenable to mathematical treatment. Laplace's law of density is a case in point. Suggested by the nature of the analysis it was found to fit in admirably with astronomical

phenomena. But Sir George Darwin has shown that other assumed laws of density are equally admissible from this point of view.

The Rev. Osmond Fisher's cumulative argument[1] against the solidity of the earth, and in favour of its fluidity, is open to the same criticism in more than one particular. The general argument against the special kind of solidity assumed is physically sound; but it is by no means certain that the distributions of density and temperature involved in this assumption correspond with the reality. Again, all through Fisher's discussion there is implied a conception of solidity as something essentially distinct from fluidity. And yet under the great stresses which must exist in a cooling sphere the size of the earth have we any reason to expect other than a very plastic condition of matter? We can imagine a gas above its critical temperature to be condensed to the density of an ordinary surface rock. It cannot be called a liquid, and yet it will certainly not behave like an ordinary gas. In like manner it is permissible to regard the interior parts of the earth to be plastic under the action of long-continued forces, and yet highly elastic to rapid vibratory motions.

When this view is taken, and when we assume a more uniform gradient of temperature throughout the mass than that originally worked out by Kelvin and adopted by Fisher, we soon realize the doubtful nature of the conclusions arrived at by the latter. Kelvin assumed that the earth solidified at a practically uniform temperature throughout its mass, and then began to cool by radiation and conduction. Rapid gradients of temperature would be found only in the upper layer of some 200 miles thickness. As a basis for estimating the superior limit to the age of the habitable earth Kelvin's assumption may be granted; but when the same assumption is made the foundation for precise calculations of the amount of contraction during geological time we seem to be on much more insecure ground.

With an appreciable gradient of temperature down to great depths the cooling will take place throughout a large part of the mass of the earth; and if the material should be

[1] See *Physics of the Earth's Crust*, second edition, 1889.

near its melting temperature there may be considerable changes accompanying this cooling process. Our ignorance of the coefficient of expansion under these critical conditions, and of the conductivity of the material at the various depths, absolutely debars us from attempting any clear-cut mathematical investigation. All we can do is to weigh the possibilities and point out the probabilities. This has been done in an interesting manner by T. C. Chamberlin,[1] who gives reasons for supposing that the gradient curve of internal temperature may be the fusion curve of the material. The following table contains the temperatures calculated from this point of view for successive tenths of the earth's radius along with the corresponding pressures and densities according to Laplace's law of density.

CHAMBERLIN'S TABLE (ABRIDGED) OF PRESSURES, DENSITIES, AND TEMPERATURES

| Distance from Centre in earth-radius | Pressure in Millions of Atmospheres | Density | Temperature in degrees Centigrade | Product of Distance and Temperature |
|---|---|---|---|---|
| 0·0 | 3·1 | 11 | 20000 | 0 |
| 0·1 | 3·0 | 10·8 | 19600 | 1961 |
| 0·2 | 2·8 | 10·5 | 18600 | 3722 |
| 0·3 | 2·5 | 10 | 17000 | 5100 |
| 0·4 | 2·2 | 9·26 | 14800 | 5936 |
| 0·5 | 1·7 | 8·39 | 12250 | 6125 |
| 0·6 | 1·3 | 7·38 | 9400 | 5616 |
| 0·7 | 0·87 | 6·28 | 6350 | 4445 |
| 0·8 | 5·1 | 5·13 | 3500 | 2776 |
| 0·9 | 2·15 | 3·95 | 1100 | 999 |
| 1 | 0·0 | 2·8 | 0 | 0 |

I have added a last column giving the products of the distances into the corresponding temperatures. In the case of a cooling sphere it is this product of distance into temperature which determines the nature of the process. Beginning with value zero it passes through a maximum at distance 0·5 from the centre, attains its maximum gradient or rate of change about the distance 0·8, and ends with the value zero. According to the laws of the conduction of heat the temperature will diminish at smaller distances than the critical distance just named, but will increase

[1] See Chamberlin and Salisbury's *Geology*, vol. i, pp. 538–541.

at greater distances. The central parts within a radius of 3200 miles will cool and part with heat to the outer shell of 800 miles thickness. This outer shell, with the exception of the surface layers, will tend to rise in temperature. In other words, the cooling will occur mainly at the lower depths. Presumably the volume contraction due to loss of heat will be more pronounced there also. This will relieve the pressure in the region separating the nucleus and shell and produce in the latter a system of elastic stresses, consisting of radial tension and tangential pressure. The plastic solid will flow towards the lower depths and the thermal contraction will be transmitted as an elastic contraction up towards the surface regions. In this process the more fusible material will be in large measure pushed upwards in the manner described by Chamberlin, and may well be imagined as giving rise to the upward tendency which has its final outcome in volcanic eruptions.

It can be shown that the loss of gravitational potential[1] energy due to a radial contraction of the earth of one centimetre would supply fully 300 times as much heat as the earth is known to be losing by radiation in one year. If we are to explain the irregularities of the earth's surface, the mountain ridges, the ocean deeps, the buckling and puckering of strata, as due in the main to stresses called into play by the contraction of a cooling globe, we must minimize the gravitational effect as much as possible. We must assume that throughout geological time the material of the earth has suffered very slight compression because of the gravitational attraction of its parts. In short, at all depths below a certain value the material is so powerfully compressed as to be incapable of much further compression. The contraction due to cooling is, however, quite a different matter; and with the temperature gradient approximately similar to that calculated by Chamberlin and

[1] See a paper by George Romanes, C.E., on The Cause of the Earth's Internal Heat, with notes by A. Gray and C. G. Knott. *Proceedings of the Royal Society of Edinburgh*, vol. xxiv, p. 415, 1903. Also T. J. See, *Astronom. Nach.*, Nos. 4053, 4104.

Lunn the real cooling in the sense of lowering of temperature will take place in the deeper parts of the earth.

We have no reason to suppose that the earth's surface was at any time of its history even approximately uniform. The broad distinction between continental domes and ocean bottoms seems to have existed through geological time, and indicates a difference in densities between the land-forming rocks and the material underlying the ocean waters. The cooling and contraction of the deeper regions as imagined above might well tend to accentuate this difference, the heavier material being drawn deeper than the lighter, and pushing the latter relatively upwards.

It is generally admitted by geologists and physicists alike that the present unequal distribution of land and water over the earth's surface is an original feature of the earth when first the solid crust formed over it. The broad feature may be expressed by saying that continents are diametrically opposed to oceans, and that the water surface greatly preponderates in the southern hemisphere. The distribution of the water surface indicates that the earth is not symmetrical, but that the centre of attraction is displaced from the approximate geometrical centre somewhat towards the southern hemisphere.

A question of interest is, when did this asymmetry come into existence? It is pointed out by Jeans[1] that if the earth cooled and consolidated with change of the elastic constants, the equilibrium configuration for the solid earth would be different from the equilibrium configuration for the fluid earth. The result would be the setting up of great stresses throughout the mass, producing ruptures and displacements with an approximation to the equilibrium form. The final configuration would probably not be symmetrical, but would retain some trace of the initial asymmetry imposed upon the fluid mass by the complex conditions of its evolution. One of the modes of vibration gives a pear shape, which may be regarded as a possible form from which the earth began its existence as a practi-

[1] See his paper on the Vibrations and Stability of a Gravitating Planet. *Philosophical Transactions*, vol. cci A, 1903.

cally rigid solid. Jeans found geographical support for the pear-shape theory in the distribution of continents and deep sea floors; but his earlier representation with Greenwich as the pole of the broad part of the pear he gave up in favour of the representation given by Professor W. J. Sollas. Sollas places Africa—the continent of highest mean height above the sea level—at the broad end of the pear, the stalk end being submersed in the Pacific Ocean. The axis of symmetry of the pear-shaped figure passes through a point whose latitude and longitude are respectively 6° N. and 30° E., and corresponds with the greatest diameter of the earth. The general features from this point of view are a land hemisphere and a water hemisphere, separated by a relatively narrow ridge of land and a narrow strip of sea. Jeans remarks that 'the fact that Africa is surrounded by a belt of seas, and this again by a belt of land before the Pacific is reached, points perhaps to a bodily subsidence of the blunt end of the pear', thus displacing the centre of gravity and causing the submergence of the stalk protuberance. This subsidence would tend to produce a line of fracture, which may possibly be represented by the mountain chains and islands bordering the Pacific. Along this great fold of the earth's surface active movements are still in progress, culminating every now and again in volcanic eruptions and destructive earthquakes, with subsiding deeps and great sea waves.

By this ingenious theory, which has gradually evolved itself mainly through the mathematical investigations of Sir George Darwin and Professor Jeans, certain broad features of the world's contour are brought into close connexion with the mode by which our planet passed from the essentially fluid condition to the present state of practical rigidity. Other natural sculptors have been at work, notably denudation and deposition and subterranean explosions; and down in the heart of the earth thermal and physical changes have been producing their own effect on the surface configurations of which alone we have any real knowledge. In the earthquake we have a phenomenon which originates in regions far beyond our ken, but which

produces unmistakable effects. From a careful study of these effects the seismologist endeavours to discover the ultimate as well as the immediate causes; and in the preceding pages I have tried to indicate some of the many ways in which this scientific purpose has been carried into execution.

# INDEX

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